# THE JOURNAL OF PARAPSYCHOLOGY

Volume 14 MARCH, 1950 Number 1

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A SCIENTIFIC QUARTERLY DEALING WITH EXTRASENSORY PERCEPTION, THE PSYCHOKINETIC EFFECT, AND RELATED TOPICS

DUKE UNIVERSITY PRESS
DURHAM, N. C.

# *The* Journal *of* Parapsychology

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This JOURNAL is published on the fifteenth day of March, June, September, and December by Duke University Press, Durham, North Carolina.

Contributions submitted for publication and all editorial communications should be addressed to the Managing Editor, Dorothy H. Pope, College Station, Durham, North Carolina. Correspondence with the editors is advised before submitting articles other than reports of experimentation. Since it does not forward manuscripts by registered mail, the JOURNAL cannot guarantee that they will not be lost in transit, and contributors are urged to keep copies of their papers. All contributions should be typewritten, double spaced. References should be given in the form adopted by the JOURNAL.

Reprints may be ordered when the proof is returned. The JOURNAL will bear one half the cost of reprints up to two hundred copies. Correspondence concerning subscriptions, change of address, back numbers, and other business communications should be addressed to Dorothy H. Pope, College Station, Durham, N. C.

The subscription price is $5.00 a year; single current numbers $1.25. The rate for back volumes is $6.00; for single numbers $1.50. Missing numbers will be supplied free when lost in the mails if written notice is given within one month of the date of issue. All remittances should be made payable to the JOURNAL OF PARAPSYCHOLOGY.

Subscriptions may be obtained in Great Britain through the Cambridge University Press, Bentley House, Euston Road, London, N. W. 1.

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Entered as second class matter May 21, 1942, at the post office at Durham, N. C., under the act of March 3, 1879.

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*The* Journal *of*
# Parapsychology

Volume 14 MARCH, 1950 Number 1

## EDITORIAL: PUBLICATION POLICY

THE RIGHTS of readers and the responsibilities of editors are matters on which there has not been much clarification in any field of interest. No one can speak with authority on such debatable topics but we can, at least as far as our field of parapsychology is concerned, raise the question and put on record what our own aims and principles are.

Probably few readers are aware of the extent to which their reading is selected and censored for them by the editors of the periodicals they read. In view of the fact that most of the educational activity of the world today functions through the printed, and of course *edited,* word, it is of the greatest importance to all of us to consider whether the editorial judgments exercised are what we want them to be.

It is, to be sure, another question what the reader clientele can do to keep the editorial function on the level of the highest principles. In the case of technical periodicals, where open competition is not much of a factor, there is ordinarily little protection against the unwise use of editorial authority. The readers, contemporaneous as well as future, have to take what the editors decide is good for them. When, for example, a new discovery is made and reported in a manuscript submitted to the editor, the readers hear about it *only* if the editor is ready to accept the article for his journal. If he is not, it is likely they will never know that they might

have had a chance of reading about it. In some cases, it is true, new developments may by-pass editorial rejections by way of the more independent writers and commentators of the daily press and radio and through other avenues that now reach the general public. Parapsychology found this by-pass, largely with the help of the professional science writers, but this way is not open to material that is highly technical or that lacks popular appeal for the general reader.

The worst feature of this problem of editorial responsibility is that the editors themselves are likely to be at a loss to know how to judge the merits of a report of research on a debatable issue. In general, if a paper raises no fundamentally new questions it can be cleared readily enough. *But if its findings do not square with current concepts* of the field and yet the report meets all the editorial standards, the editor is hard put to find a basis of judgment. He will probably have the manuscript read by an "authority" in the special research area dealt with, but the "authority" has the same problem as the editor if the new claim is really unorthodox. And since authority, like editorship, makes for conservatism, the paper is likely to be rejected. (The editor may or may not divulge the real reason to the author.)

The main reason for concern here lies in the fact that the readers never know anything about these transactions; they have no voice in the decision made and of course no opportunity to register their reactions. Thus even though the editor may try conscientiously to guard against misjudgment—may, for example, build up a staff of expert advisers who share the responsibilities for the more difficult decisions—the power of even the least dictatorial of editors is pretty nearly absolute in his domain.

The responsibilities of editorship tend naturally to breed caution and conservatism. For one thing, the editor of a scientific journal may be criticized for what he publishes, but he seldom is for what he turns down; for almost no one knows he has turned anything down. Moreover, he does not have very much confidence in his criteria for judgment as to whether or not a new claim or idea deserves a hearing. This indecision makes him more cautious and less liberal. These circumstances, and a conscientious editor's tendency to identify himself with his journal and to feel responsible for

every page, make him anything but venturesome with regard to any new ideas proposed for his columns.

The general interest of the reader, then, would seem to require two lines of correction in the editing of the kind of publications we are discussing. The first is a shift of emphasis in the matter of editorial responsibility away from that of trying to protect the reader against a *wrong* idea, and toward better insurance against allowing him to *miss* any good ones. All that is needed is more editorial recognition of the fact that readers, as well as editors, should be able to think for themselves and should want to have left to them some of the adventure of deciding doubtful issues. This correction will then occur as a matter of course. With the assumption of a mature-minded reading clientele, authoritative editorship would be out of place and entirely presumptuous. If the editor could be led to become more of an explorer and to search for new developments on behalf of his readers, his public would miss less of what is new and interesting and the pioneering author would seldom be frustrated. Editorial dictatorship would then give way to a democratic policy of allowing a claim to stand on its merits.

The second progressive measure the editor could take is that of acquiring a more efficient standard of judgment than mere consistency. There are other standards, of course; otherwise the new (and always inconsistent) findings of science would never have been accepted. There are ways to evaluate properly a scientific paper dealing with a project that produces findings inconsistent with existing knowledge. It is unfortunate, however, that most of us lazily put the issue aside to await further work along the same line or for an "authority" in the field to approve the new claim. But the keen inquirer, whether editor or reader, who wants a sound estimate of the work before him, needs to hold the research up against the same pattern of appraisal the original research worker himself used (or should have used) in guarding against error in the conduct of the research. Naturally the more clear one is about scientific method, the better he can evaluate a new study. The point to be emphasized is that the procedure of evaluation is no different for the author, the editor, or for the reader, provided the reporting is adequate.

Thus, in a proper report, the editor can go over in review all the essential steps through which the original research worker went as he did his work. If places are found in the paper where a necessary step was omitted or a counterhypothesis was not given due attention, judgment has to be suspended at the end of the reading. Obviously, no conclusion is stronger than the weakest link in the chain of development by which a problem is solved. If any such serious gaps or flaws in the paper are found, the editor's procedure is then simple and objective: he asks the author for a more complete report covering the points that are missing. If they are not available, then the work itself is at fault and repetition of the research on a more adequate basis is to be recommended. It is not a question of the truth of the research conclusion, but of the completeness of the methodological case. Here the editor is not closing the door to the new discovery but is helping the author to strengthen his case in presentation.

If there is no serious editorial criticism of research or reporting, then the paper should by all means be released, regardless of the radical nature of the conclusions or their inconsistency with existing concepts. It may well be that the more intellectual uproar the report causes, the greater will be the ultimate significance of the findings.

We are assuredly using the word science in vain if we cannot measure the soundness of our findings by means of the very methods which gave them to us, and thus avoid having to consider the more superficial criteria of familiarity, consistency, and the like. For everyone concerned in the critical appraisal of a piece of work, the outline of the procedure of assessment should be the same. If, as we more than suspect is the case, scientific method is not sufficiently well known for such general application, then obviously a beginning should be made on promoting a wider acquaintance with it. This educational step could well begin with the editors themselves.

* * * * *

These principles are general and of wide application, but we are naturally interested mainly in their special bearing on the field of parapsychology. We believe they will not find anywhere a larger measure of adherence than among the staff of this JOURNAL; but we realize too that there is need for an express declaration of policy and a review of editorial objectives and procedures from time to

time if we are to obtain the best cooperation and counsel from what we might call our journal community. There seems to be no satisfactory way in which to acquaint the reader with the specific decisions involved in each acceptance and rejection, each alteration of policy, or every issue that challenges the customary standards. Still, we can come closer to the ideal of good reader-editor understanding by putting on record the general principles to which the editors try to adhere.

Such an occasion allows also for a review of some of the more pressing troubles of the editorial staff. Perhaps the problem that is of most concern both to readers and editors of the JOURNAL OF PARAPSYCHOLOGY is that of trying to keep the periodical technical on the one hand and of attempting to make it more popular on the other. The editorial aim is to keep the JOURNAL as technical as accuracy and completeness of reporting require; but in view of the fact that there is no sustaining organization and no official clientele sufficient to support a scientific periodical in its field, the JOURNAL must, for the time being at least, serve also to carry the findings of parapsychology to an intelligent lay audience which cannot be expected to keep up with the technicalities associated with the research. As much of a concession is made to readability, therefore, as can be made without endangering the primary requirement—that is, scientific reporting. This is the only solution we can find at the present stage. With the use of abstracts giving the general content of each article in more simple language, and with special editorial attention to the more technical reports, the editors hope that their objective can be moderately well achieved.

The plan has been to emphasize reports of research in parapsychology as the main content of the JOURNAL. The research is primarily experimental in character, of course, but not completely so. Discursive and interpretative articles are welcome when they concern the research findings of the field. Critical articles are also welcome, and, like any other type of paper that is relevant to the research problems dealt with by the JOURNAL, they will be returned to the author for rewriting or amplification only if they fail to make a clear, logical case. It is, in many instances, very difficult for the editors to decide between two alternatives. They may find it more profitable for an article to be published with a fallacious or incom-

plete argument, thus submitting the readers to a controversy which runs through successive numbers and which presumably will end up in the correction of the original statement, or it may be the duty of the editors at the risk of overstepping their bounds to call an author's attention to weaknesses in his case and ask for a perfected argument in the original article. It is probably the wisest whenever possible to leave to the author, especially if he is a mature and trained research man, the decision whether to publish his article at his own risk or whether to accept the recommendation for rewriting. Rejection in such cases could be dangerous. The device of publishing editorial comments can be used if necessary to clarify debatable issues.

Difficulties frequently arise for the editors, too, in trying to decide whether to publish a report of a research that failed to make the contribution its author may have intended. Sometimes an experiment which was intended to throw some light on the occurrence or nature of psi phenomena ends with results explainable by chance. If the author has no illuminating observations or conclusions to relate and no novel method to describe, the editors feel that there is not space or reader interest enough to justify the publication of a report unless there is the possibility of learning something from such a failure. There are obviously too many ways of going wrong in the search for delicate capacities such as ESP and PK for us to draw any conclusions from a failure to obtain significant results. It is worth while, however, to give brief mention of a study that has suggestive results or presents some incidental outcome of interest either to the general reader or the special research worker.

Since some people are under the wrong impression that failure to report a completely fruitless research somehow invalidates the conclusions drawn from significant studies that have been reported, some further indication is needed as to what should, and what should not, be done about reporting such chance results. The methods of statistical evaluation applied to the results of an experiment require, of course, the inclusion of all the relevant data, *all of the experimental material obtained under the conditions of the particular research.* Any other policy would be misleading to both the author and the reader. Each research, however, must rest on its own base, and the value and finality to be attached to a given research report is not seriously affected by any number of experiments done by

others with what are necessarily many other variables in the experimental situation. It is true, two successful experiments may to some extent support each other if there is sufficient similarity, but an experiment giving chance results does not properly detract anything from another in which the results were significant unless all the conditions of the two are identical. The experiment with insignificant results warrants no general conclusions except where all the variables are controlled, and that is never the case in any research with human subjects.

The pooling of all combinable experimental results is occasionally justifiable to a limited extent and for certain purposes. For example, at the time of the publication of *Extrasensory Perception After Sixty Years,* a grand survey of the total available findings of the known researches in ESP was made and reported, together with as much pooling of results as was possible. It was found that an incredibly large and wholly impossible amount of ESP experimenting would have to be done at the level of chance to dilute and neutralize the results of the findings available at that date (1940). And since that publication a great deal more has been accomplished to buttress the case against the hypothesis of chance. The statistical issues have been repeatedly fine-combed and the main emphasis has long since passed on to the problem of the nature of psi phenomena, with the establishment of their occurrence now taken for granted.

Editorial assistance should cover not only the adequacy of general experimental method and logic, but the special techniques as well. The editors of the JOURNAL have assumed as a part of their responsibility a review of the statistical handling of material published and have been able to do this through the expert counsel and assistance of Dr. Greenwood and Dr. Greville. Similarly, the exercise of a supervisory responsibility for rechecking data and computations has been practiced (except in rare instances when this is not possible).

Whatever policy and practices are adopted, however, there are bound to be limitations and cases where compromise is necessary. An author may have difficulty in accepting our suggestions or may be inaccessible (as in the case of the recent publication of the translation of Bechterev's article). If, however, the reader is duly in-

formed of these limitations he is not likely to have difficulty in making his appraisal of the report.

The greatest shortcomings to which the editors of this JOURNAL confess would appear to belong on the side of omission. Our chief difficulty concerns the growing amount of accumulated research material that fails to get reported because of limited research personnel, pressure of more rewarding activities, and other human factors (such as disinclination on the part of those who did the experiments to writing the reports, hesitation to become publicly identified with parapsychology, etc.). In the Parapsychology Laboratory alone there are some dozens of smaller researches still unreported, most of them making some contribution of sufficient value to qualify for admission to the JOURNAL if properly reported. No satisfactory way is at present in sight to bring this accumulation to publication. This situation derives from limitation of help and is just a part of the more general problem of the totally inadequate support being given to this branch of science.

We have had one important advantage in parapsychology, however, though it has not always been easy to appreciate it—we have been compelled at all times by vigorous criticism to look well to our methods. More conscious of our hazards than are workers in most of the sciences, we have developed an interest in the reliability of research methods that other inquirers have not needed to consider. We have also been made more than ordinarily conscious not only of research methods, but of editorial problems as well, and we appreciate the importance of free channels for the outlet of advances in thought. Such experiences have given us some insights we should not otherwise have gained and some suggestions of value, we believe, even far beyond the boundaries of our research domain.

J. B. R.

# A NEW SCALE FOR SEPARATING HIGH- AND LOW-SCORING SUBJECTS IN ESP TESTS

By Betty M. Humphrey

**ABSTRACT: From a study of the answers that subjects in three ESP card-test series gave in filling out the Stuart Interest Inventory, Dr. Humphrey found 14 specific items in the inventory that were most effective in separating the high-scoring and low-scoring ESP subjects in the three series. On the basis of these 14 items a new interest scale was constructed. Scores above a certain point on the scale were designated as "high," meaning that subjects with such scores were expected to give positive ESP results; scores below this point on the scale were called "low," indicating that subjects with such scores were expected to give ESP results below mean chance expectation. The new scale was tested by applying it to the interest tests of the 426 subjects in 13 available clairvoyance card-test series. Analysis of the data showed that the group of subjects with "high" scores on the new inventory scale gave total ESP results that were positive, while those in the "low" group gave total ESP results below mean chance expectation. The difference between the two groups was statistically significant. Moreover it was found that the majority of subjects in the "high" group gave positive ESP deviations while the majority of those in the "low" group gave negative deviations on the ESP test. The results of this study are discussed in relation to other studies of the data in these same 13 series.—Ed.**

## Introduction

In the search for measures capable of separating high-scoring subjects in ESP tests from low-scoring ones, at least two different methods of approach are available. One involves the use of already established psychological tests; the other involves the construction of tests designed solely for the purpose of separating high- and low-scoring ESP subjects. In following the first method, the experimenter starts out with a particular method of personality measurement that has already been used and makes a study of how this aspect of personality is related to ESP ability. The experimenter in this case needs to select the particular personality test that he wishes to use for measuring the personality attributes he intends to study, just as he needs to select the kind of test he wishes to use for measuring the ESP ability of his subjects. For example, in a number of experiments (**3, 4, 8**) the clinically useful "expansion-compression" criterion for judging form qualities in freehand drawings

has divided subjects into two groups having quite different scoring levels in ESP drawing tests. Certain measures of personality based on the Rorschach Ink-Blot Test were found similarly effective in ESP research by Schmeidler (**5**). In these and in several other researches, the method of studying relationships between ESP performance and personality characteristics has been to see whether ESP scores are related in any consistent way to certain known measures of personality.

In following the second method of approach to the problem of what characteristics distinguish "good" ESP subjects from "poor" ones, the investigator studies the responses of high-scoring ESP subjects and of low-scoring subjects to questionnaires or other types of psychological tests in order to determine *empirically* wherein the two groups differ. In this case, the experimenter may use available psychological tests or he may devise tests or questionnaires of his own; the primary question he seeks to answer is whether or not performance level in ESP tests is related to the way subjects respond to the questionnaire or psychological test. If, for example, certain responses were found to be typical of subjects with high ESP scores and other responses characteristic of low-scoring ESP subjects, a new checking scale for the questionnaire could be devised. In subsequent use of the scale one would have a means for predicting whether a given person could be expected to achieve high or low ESP scores. This simplified version of a complicated method will serve to indicate briefly the approach used in the studies to be reported in the present paper. In this approach, the emphasis is on the discovery of differences, if there are such, in the way high and low ESP scorers respond to a questionnaire or inventory; the question of the definition of the personality attributes that might account for any differences so discovered is relatively secondary and must be left for a later stage of the research.

In recent issues of the JOURNAL there have been several reports on the Stuart Interest Inventory which the late Dr. C. E. Stuart devised a few years ago (**1, 2, 7**). The inventory consists of sixty items commonly of interest to college students. Subjects serving in a number of ESP experiments were asked to respond to each item by indicating whether they liked the activity or interest listed ("L"), liked it very much ("LL"), were indifferent to it ("I"),

disliked it ("D"), or disliked it very much ("DD"). The interest inventories of some of the subjects were analyzed in order to see if certain of the sixty items were answered differently by high-scoring and low-scoring subjects. The analysis used was the same as that employed by Stuart on the interest tests of some of his subjects who had taken part in ESP drawings experiments. The results of Stuart's study (unpublished) were not significant, but they were encouraging. Therefore, I decided to follow the same procedure in analyzing the interest tests of subjects in ESP card experiments.

In previous reports on the interest inventory there were sixteen clairvoyance card experiments in which interest tests were obtained from the subjects (**1, 2**). The first three group-test series (known as Series S1, S2, and S3 in the reports) were chosen as the basis for the analysis. Briefly, the method was as follows: The answers given on the interest test by the high-scoring subjects in these three series were compared with those of the low-scoring subjects. As will be explained in detail below, it was found that fourteen of the items were answered differently by the two groups of subjects. On the basis of these answers a scoring scale was constructed, and certain scores were designated as predictive of high or low scoring on clairvoyance card tests. The effectiveness of the new scale was tested on the remaining thirteen series, which had in no way entered into the construction of the scale.

## Method of Scale Construction

The three clairvoyance card-test series, S1, S2, and S3, which were to serve as the basis for the scale construction, had been carried out by Dr. Burke M. Smith with his students during the same two-day period. These series have been described in the other reports (**1, 2, 6**) and the details will not be repeated here. They were chosen to serve as the "criterion group," that is, the group upon which a new scale would be constructed, because they formed a relatively homogeneous batch of data: they were carried out with similar classes of students by the same experimenter during the same temporal period and were conducted with the same experimental procedure. They were also the first three of the long series of group clairvoyance card experiments carried out by members of the Laboratory staff.

From the total of 91 subjects making up the three series, two groups were first chosen arbitrarily for special study: (1) the subjects who averaged six hits or above for the four runs given each subject; and (2) the subjects who averaged four hits or fewer for the four runs. In the first, or high-scoring group, there were 21 subjects, while in the second, or low-scoring group, there were 24.

The interest inventories of the high-scoring subjects were analyzed item by item in order to see how many answers of LL, how many of L, and so forth, were given to each item. The same analysis was then made for the low-scoring subjects. The percentage of high-scoring subjects and the percentage of low-scoring subjects who gave each of the five answers was then computed. For example, on item No. 5 (Nature Study) the percentages for the five answers were as follows:

| | LL | L | I | D | DD |
|---|---|---|---|---|---|
| High Scorers ..... | 10% | 24% | 28% | 33% | 5% |
| Low Scorers ..... | 8% | 29% | 59% | 4% | 0% |

From these figures, we see that 38% of the high scorers indicated dislike ("D" and "DD") of the item, while only 4% of the low scorers disliked it. Thus this item would appear to distinguish between high- and low-scoring subjects; the figures suggest that if a person indicates dislike of nature study the chances are that he will be a high-scoring subject.

A similar evaluation was made of all of the items in the inventory. Arbitrarily it was decided to select for inclusion in the new scale all of the items in which there was at least 20% difference in the number of high- and low-scoring subjects giving specified answers. (For example, in item No. 5 above, the difference in percentage of high and low scorers answering D and DD was 34.) Percentages for combinations of LL and L, or DD and D were considered (as was the case in the illustration above) as well as the percentages for each of the five possible answers.

In all, fourteen items were chosen by this criterion from the original sixty. The items, together with the specific answers discriminating high and low scorers, are as follows:

| | High Scorers | | Low Scorers |
|---|---|---|---|
| *Like:* | History (LL-L)<br>Dramatics (LL)<br>Bridge (LL-L)<br>Salesmen (LL-L)<br>Formal Affairs (LL-L) | *Like:* | Military Drill (LL-L)<br>Boxing (LL-L)<br>Animal Zoos (LL-L)<br>Geography (L) |
| *Indifferent to:* | Cowboys<br>Stamps<br>Birds | | |
| *Dislike:* | Nature Study (DD-D) | *Dislike:* | Algebra (DD-D) |

These items were empirically derived; no attempt was made to select a group of items which fitted into any preconceived rationale.

The next step was to set up a scoring system for the items. For ease in checking, it was decided to give one point for each answer favored by the *high*-scoring subjects. For example, in subsequent application of the scale, a score of +1 was given to subjects answering L or LL to the "History" item. On items which were liked by the low-scoring subjects, a score of +1 was given for any answer *other than* L or LL. Thus, for example, a score of +1 was given to subjects who answered the "Military Drill" item with I, D, or DD, since subjects who liked that item were the low scorers. With the one item which the low scorers disliked (Algebra), a score of +1 was given for any answer other than D or DD. The complete scoring scale as it was actually used in the present study is shown below. One point was given for the answer or answers indicated after each item.

FOURTEEN-ITEM SCALE

| Items | Answers |
|---|---|
| Algebra | LL-L-I |
| Geography | LL-I-D-DD |
| History | LL-L |
| Nature Study | D-DD |
| Dramatics | LL |
| Military Drill | I-D-DD |
| Boxing | I-D-DD |
| Bridge | LL-L |
| Salesmen | LL-L |
| Cowboys | I |
| Animal Zoos | I-D-DD |
| Stamps | I |
| Birds | I |
| Formal Affairs | LL-L |

The next step followed in this study was to score interest test records for *all* of the subjects in Series S1, S2, and S3 using the 14-item scale. The scale was applied to the records of all 91 subjects (not just the 21 high scorers and 24 low scorers) in order to provide a broader basis for determining what scores on the scale could be considered indicative of high ESP scores and of low ESP scores.

Each inventory was given a score (from 0 to 14) indicating the number of items answered according to the responses listed on the scale. The scores were then arranged in order of decreasing magnitude, and the ESP results for the subjects at each interest scale score level were compiled. The following distribution of the 14-item scale scores and associated ESP results was found:

| 14-Item Scale Score | Number of Subjects | ESP Results | |
|---|---|---|---|
| | | Runs | Dev. |
| 13 | 1 | 4 | + 8 |
| 12 | 0 | 0 | 0 |
| 11 | 2 | 8 | +14 |
| 10 | 10 | 40 | +25 |
| 9 | 7 | 28 | +11 |
| 8 | 22* | 86 | +24 |
| 7 | 13 | 52 | − 5 |
| 6 | 12 | 48 | −15 |
| 5 | 12* | 46 | −36 |
| 4 | 7 | 28 | −21 |
| 3 | 2 | 8 | − 8 |
| 2 | 2 | 8 | − 9 |
| 1 | 1 | 4 | − 2 |

* One subject made only 2 runs instead of the usual 4.

From an inspection of these figures, it may be seen that, as a group, subjects with 8 or more points on the 14-item scale gave positive ESP results, while those with scores of 7 and below on the new scale gave negative deviations on the ESP test. If we add the ESP results for subjects having 8 or more points on the 14-item scale, we get a total of 166 ESP runs with a deviation of +82 and a CR of 3.18. The ESP results for subjects with scores of 7 or less on the scale amount to 194 runs, with a deviation of −96 and a CR of 3.45. The CR of the difference between the two groups of data is 4.68.

Of the 42 subjects who obtained scores of 8 and above on the scale, 67% gave positive ESP results, 2% gave zero deviations, while 31% had negative deviations on the ESP test. In the group having 7 or fewer points on the scale 35% of the 49 subjects obtained positive deviations; 2% gave zero deviations; and 63%, negative deviations. Thus we see that the majority of the subjects having 8 or more points on the scale gave positive deviations on the ESP test, while the majority of those having less than 8 points gave negative deviations on the ESP test.

On the basis of these results, it was decided in subsequent use of the scale to designate as "high" scores those of 8 and above, and to designate as "low" scores those of 7 and below. Thus groups of subjects who obtain "high" scores on the 14-item scale will be predicted to give positive results on ESP tests, while subjects with "low" scores on the scale will, as a group, be expected to give a negative deviation in ESP tests.

## Description of Test Data

In order to test the effectiveness of the new scale, we must apply it to data which are as comparable as possible to those from which the scale was derived. It has been mentioned before that the three series upon which the construction of the scale was based were the first of a number of experiments in which groups of subjects were given ESP tests. The three series were followed by twelve additional clairvoyance group experiments. At the time of writing, interest inventory data are also available for one experiment in which subjects were tested individually for clairvoyance. Although the twelve group experiments were carried out for different purposes and some of them involved the use of somewhat different procedures than were used in the first three series (the criterion series), they do afford the best large collection of data available for testing the effectiveness of the new scale; and, like the criterion group, they are all series in which subjects were given group tests of clairvoyance. For the sake of completeness, the scale will also be applied to the one series of clairvoyance tests administered individually to subjects who also filled out the interest inventory. Thus, in all, we have thirteen series to which to apply the 14-item scale. None of the series were in any way involved in the construction of the scale.

The procedures involved in the various series and the precautions employed in testing have been described in other reports (**2, 6, 8**) and the details will not be repeated here. In the series known as S4 Sm, S4 Ma, S5, S6, S7, and S8, Dr. Burke Smith acted as experimenter (as he was also for the series in the criterion group). In the series called R6, R7, R8-A, and R8-B, Dr. J. B. Rhine served as experimenter. Miss Elizabeth McMahan conducted the individual tests which make up Series M1. I was the experimenter in two series known as H2 and H4. The reader need not be concerned with remembering the various series names—they are repeated here principally for the student who may wish to refer to previous accounts of these series in other connections.

In all of the series conducted by Smith except one (S7), the procedure used was the BT method in which subjects recorded their respective calls for each card as it was held apart from the deck by the experimenter. The BT procedure was also used in Series M1, R6, and H2. In all other series except Series R7, the subjects were asked to call "down through" the pack of cards, which was not touched until the run was completed. A special technique was used in Series R7, in which subjects called the order of cards which were individually enclosed in black opaque envelopes.

In all series the order of the cards in the target decks was not known to the experimenter, and in all series the cards were screened from the subjects' view.

The interest inventories of the subjects in these thirteen series were checked according to the method described in the previous section. If the subject obtained a score of 8 or more as indicated on the scale, he was placed in the "high" group—the group expected to give a positive deviation on clairvoyance tests. A score of 7 or below on the scale placed the subject in the "low" group—the group expected to give a negative deviation on clairvoyance tests.

All ESP test data as well as all inventory data have been completely rechecked.

## Results

In the thirteen series upon which the independent test of the 14-item scale was made, there were a total of 185 subjects who were placed in the "high" group. These subjects contributed a

total of 473 ESP card runs with a deviation of +90. Altogether the 241 subjects in the "low" group according to the scale gave a total of 594 runs with a deviation of −101. The difference between these two groups has a CR of 2.92. The probability that a CR this large or larger could arise by chance is approximately .0035. (Table 1 shows the results separately for each series as well as for the total.)

Table 1

ESP Results for Groups with "High" and "Low" Scores on the Fourteen-item Scale

| Series | "High" Scores | | | "Low" Scores | | |
|---|---|---|---|---|---|---|
| | Runs | Dev. | Av. Score | Runs | Dev. | Av. Score |
| S4 Ma | 18 | +12 | 5.67 | 50 | 0 | 5.00 |
| S4 Sm | 22 | +11 | 5.50 | 34 | −11 | 4.68 |
| S5 | 26 | − 3 | 4.88 | 46 | + 9 | 5.20 |
| S6 | 44 | − 6 | 4.86 | 76 | −52 | 4.32 |
| S7 | 32 | +15 | 5.47 | 48 | +17 | 5.35 |
| S8 | 22 | − 1 | 4.95 | 36 | − 5 | 4.86 |
| R6 | 42 | + 8 | 5.19 | 32 | − 4 | 4.87 |
| R7 | 15 | −10 | 4.33 | 20 | + 1 | 5.05 |
| R8-A | 66 | + 1 | 5.02 | 76 | 0 | 5.00 |
| R8-B | 84 | +32 | 5.38 | 76 | −21 | 4.72 |
| H2 | 18 | +12 | 5.67 | 38 | −22 | 4.42 |
| H4 | 40 | +18 | 5.45 | 26 | − 2 | 4.92 |
| M1 | 44 | + 1 | 5.02 | 36 | −11 | 4.69 |
| Total | 473 | +90 | 5.19 | 594 | −101 | 4.83 |
| | | CR = 2.07 | | | CR = 2.07 | |
| | | | $CR_d = 2.92$ | | | |

Thus, by means of the 14-item scale, a batch of ESP test data which as a whole gave insignificant score deviations (1,037 runs with −11) has been broken into two groups whose ESP score averages are significantly different.

In a previous section, it was noted that in the criterion series themselves the scale was far from perfect in placing subjects with positive ESP scores in the "high" group and subjects with negative ESP scores in the "low" group. The *majority* of the subjects were, however, correctly placed. Now let us see with what degree of consistency subjects in the thirteen series used to test the scale were correctly assigned to groups expected to give positive and negative deviations. In the "high" group according to the scale, there were 185 subjects. Of these 54% obtained ESP scores above mean

chance expectation as predicted; 12% gave zero deviations; and 34% had ESP scores below mean chance expectation. A total of 241 subjects were assigned to the "low" group on the basis of the 14-item scale. Only 34% of these had positive ESP scores; 15% had zero deviations, while 51% gave negative deviations. Thus we see that in these series, too, the *majority* of the subjects were correctly placed by the scale in groups expected to score "high" or "low" on ESP tests, although the percentages correctly placed are, of course, not as high as those of the criterion series.

In order to test whether or not positive and negative deviations are significantly related to ratings of "high" and "low" on the 14-item scale, we can make up a contingency table composed of the figures from which the above percentages were obtained. In the table given below, the *number* of subjects in the "high" group who gave positive deviations is entered in the upper left cell. In accordance with a practice followed previously (**1, 2**), I arbitrarily combined the number of subjects in the zero and negative deviation categories and entered this figure in the upper right cell. In the two lower cells of the table comparable figures are given for the subjects in the "low" group according to the scale. (The expected number of cases in each cell is given in parentheses underneath the observed number in each category.) The following table results:

| Subjects Separated by 14-item Scale | Subjects Separated by ESP Deviation: + | Subjects Separated by ESP Deviation: 0, − | Total |
|---|---|---|---|
| "High" | 99 (79.04) | 86 (105.96) | 185 |
| "Low" | 83 (102.96) | 158 (138.04) | 241 |
| Total | 182 | 244 | 426 |

$\chi^2 = 15.56$
d.f. = 1
P = .00008

The chi square based on this table indicates that the 14-item scale was successful in separating the subjects of group ESP tests

who made positive deviations from those with zero or negative deviations. The previous evaluation by means of the CR of the difference showed that the "high" and "low" *groups* of subjects gave significantly different average ESP scores. The chi-square method indicates further that the difference in ESP scores was due to the majority of the subjects and not to the correct placing of merely a few subjects who had outstanding deviations, either positive or negative.

## Discussion

By means of an empirically derived scale of fourteen items the subjects in thirteen clairvoyance series were separated into two groups whose average ESP scores were significantly different. Moreover, the majority of the subjects placed in the "high" group by the scale gave positive ESP scores, while the majority of those assigned to the "low" group gave negative deviations on the ESP test. Thus it appears that there actually is some consistent difference in the way high- and low-scoring ESP subjects respond to the fourteen items.

The items included in the scale were selected on an empirical basis, there being no attempt made to select items which fitted any preconceived rationale or which seemed logically to belong together. While the scale was designed solely to separate high- and low-scoring ESP subjects, perhaps from a closer inspection of the items we may glean some suggestions as to what aspects of personality are being tapped by the scale. At the present stage, we can only try to formulate hypotheses as to what are the significant characteristics of high- and low-scoring ESP subjects as indicated by the specific answers on the scale; only by additional research can the validity of the hypotheses be checked. Noting that the high scorers like bridge, formal affairs, history, salesmen, and dramatics, we might hypothesize that high scorers tend to be much more sociable than the low-scoring subjects who are either indifferent to or dislike the "socially oriented" pursuits mentioned. The high scorers tend to be indifferent to activities which might be considered "solitary," such as bird study and stamp collecting, and they indicate dislike of nature study, another relatively "solitary" activity. Perhaps we can legitimately sum up these various likes and dislikes by

saying that high scorers on clairvoyance tests tend to be more interested in people than in things. This hypothesis can be experimentally tested by noting subjects' reactions to psychological tests or situations designed to measure "sociability," "social extraversion," or related characteristics going under a variety of names in psychological jargon.

It is hard to try to make any generalization about the low-scoring subjects. Two of the items they tended to like, boxing and military drill, might be classed as "aggressive." Whether these subjects are actually aggressive in interpersonal relations, or whether their liking for these items is indicative of repressed aggression which achieves some measure of release in watching the activities in question, or whether there is any relationship at all between aggression and low-scoring on ESP tests will have to be determined by further research.

It is difficult to find any rationale to cover the appearance in the scale of such items as algebra, geography, and cowboys.

This is only the first report on the scale and on the use of this method for determining characteristics of high and low scorers in ESP tests. As such it suggests several avenues for possible research. One such avenue would be further study of the scale itself to see if it can be refined. Such study would eliminate items which are not effective; this in itself should increase the effectiveness of the scale as a whole, and it should also give us a clearer indication as to what aspects of personality are important to success and failure in clairvoyance tests.

Additional study of the fourteen items may also suggest the advisability of assigning more weight to some items; at present all fourteen items receive equal weight in scoring. If a graded weighting system were used, the scale might be more effective than it is now.

The purpose for which the 14-item scale was devised was to separate the subjects having positive deviations from those having negative deviations—not to separate subjects according to the *degree* of ESP ability shown. We cannot say that the high-scoring subjects in the criterion group and in the thirteen additional series gave indication of a greater amount of ESP ability than did the low-scoring subjects. For a number of years now it has been recognized

that significant negative deviations are as strongly indicative of ESP as are comparable positive deviations. So, by using the 14-item scale, we have not separated subjects with ESP ability from subjects with no ESP ability (as measured by their performance in card tests); but we have separated subjects whose ESP ability worked to get more than the expected number of hits from the subjects whose ESP ability worked to reject the target symbols and obtain fewer than the expected number of hits. The evidence for ESP is equally strong in both groups. These facts suggest the worthwhileness of carrying out another study in which the inventory answers of subjects whose test scores give evidence of ESP are studied in relation to the answers of subjects whose test scores give no evidence of ESP; or, roughly speaking, we might contrast the answers given on the inventory by the high and low scorers with the answers given by the "chance" scorers. If we can find differences in the way the three groups of subjects (high, low, and "chance") answer certain items, we might be able to devise a scale which would enable us to predict not only the direction of a subject's ESP deviation (positive or negative), but also the *degree* of ESP ability he will show in card tests of clairvoyance.

The main value of the 14-item scale will undoubtedly lie not in its own specific contribution but in the various leads it suggests for future research.

The thirteen series to which the 14-item scale was applied have also figured in previous researches in which two other measures were used in the attempt to separate high-scoring ESP subjects from low-scoring ones. It is interesting to compare the degree of separation achieved by the other two measures with that obtained by the use of the 14-item scale. In one research (1) subjects in these same thirteen series were separated into two groups by means of "expansion-compression" ratings based on drawings which the subjects had made during the same session in which they took the ESP card tests. The ESP results of subjects who were classified as "expansive" were compared with the ESP results of those who were rated "compressive." In another research (2), the same subjects were reclassified in terms of ratings based on their responses to all sixty items in the Stuart Interest Inventory. In this analysis subjects were designated as either "midrange" or "extreme." In the table

below, the average ESP run scores of subjects classified according to the measures just described are presented for comparison with the average run scores obtained when the 14-item scale was used.[1] (The average score expected by chance is 5.00.)

| Group | Av. Score | Group | Av. Score | Group | Av. Score |
|---|---|---|---|---|---|
| Expansive | 5.06 | Midrange | 5.14 | "High" (14 items) | 5.19 |
| Compressive | 4.96 | Extreme | 4.84 | "Low" (14 items) | 4.83 |
| Difference | .10 | | .30 | | .36 |

These figures show that the new 14-item scale has been more effective in separating high- and low-scoring subjects than either of the two measures previously applied to the same data.

Recently, a research (1) was reported in which the expansion-compression rating was combined with the midrange-extreme measure; ESP scores of subjects who received both expansive and midrange ratings were compared with those who were classified as both compressive and extreme. By means of this combination of measures a greater degree of separation of high and low scorers was obtained: expansive midrange subjects averaged 5.22, while compressive extreme subjects averaged 4.78, the difference between the averages being .44. Thus the 14-item scale has been surpassed in effectiveness only by a *combination* of two measures. Future reports will show the results achieved when the 14-item scale is used in combination with these other measures.

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[1] The figures cited here for the expansion-compression and the midrange-extreme researches do not agree with the published reports on these researches because Series S1, S2, S3 were omitted from the above figures. They were omitted in order that the data to which the other measures were applied would be comparable to those on which the 14-item scale was used. (The three series had to be left out of the 14-item scale study since the construction of the scale was based on them.) These three series had contributed a great deal to the differences obtained. Their omission here of course results in smaller differences than were reported for the total of sixteen series.

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# IMPRESSIONS OF SOME EARLY WORKERS IN THE S.P.R.

By Helen de G. Salter[1]

My father and mother, Dr. and Mrs. A. W. Verrall, were both Cambridge scholars. My father was a Tutor and Lecturer at Trinity College and, in the last years of his life, King Edward VII Professor of English Literature. My mother was one of the early students of Newnham College. Their home was in Cambridge and their circle of intimate friends included several of the founders of the Society for Psychical Research—Frederic Myers, and the Sidgwicks, as well as many of its early supporters, including Edmund Gurney. Myers, in particular, was a familiar figure to me from my earliest years; his three children were my playmates, and I spent much of my time in their house and garden. Edmund Gurney I may have seen as a very young child; in fact, I think it likely that I did see him, and I have a dim recollection that he once gave me a doll. But the only knowledge I have of him beyond what may be read in books comes from hearing my father talk of him; they had been close friends at Trinity. I remember a story my father told which well illustrates Gurney's quick, fantastic humor. A group of friends were discussing scientific problems in a desultory way, and "polarity" was mentioned. "By the way," said one, "what is polarity?" "Oh," said Gurney, "it's being different at the two ends, like a polar bear."

Sidgwick I myself never knew well (I was a school-girl still when he died in 1900), though I saw him often. Among the founders of the S.P.R. (and I reckon Mrs. Sidgwick among its founders, though she did not actually join the Society until a year or two later) it is difficult to give pre-eminence to one or another; but I think all who know the work of the S.P.R. will agree that this work could never have been done, nor the traditions of the Society so

[1] Mrs. Salter has been associated with psychical research in England since 1908 when she first became active in the Society for Psychical Research. She has served for many years as a member of the Council of the S.P.R. and is at present Hon. Editor of the *Proceedings*.—Ed.

firmly laid down (a matter absolutely vital to its aim and purpose), without the active and acknowledged support of such a man as Henry Sidgwick. Both his academic status and his personal character were an immense asset. After a brilliant career at Trinity College, Sidgwick was elected a Fellow of the College in 1859; ten years later he resigned his Fellowship on conscientious grounds, because he could no longer declare himself "a *bona fide* member of the Church of England." Most people did not take this condition very seriously, and Sidgwick has himself recorded that many of his friends were surprised that he should feel his action necessary. But Sidgwick was a man of very great intellectual integrity, deeply concerned throughout his life with the ethical problems of humanity. "I happen to care little," he wrote to a friend, "what men in general think of me individually; but I care very much what they think of human nature. I dread doing anything to support the plausible suspicion that men in general, even those who profess lofty aspirations, are secretly swayed by material interests."

After Sidgwick ceased to be a Fellow his connection with Trinity College was maintained by his appointment to a Lectureship in Moral Science, and in 1883 he became Knightbridge Professor of Moral Science in the University of Cambridge. This appointment he held until his fatal illness in 1900.

It is clear from Sidgwick's correspondence that his interest in the problems of psychical research goes back to his undergraduate days. In 1859 he joined the Cambridge "Ghost" Society, and during the following years he spent a good deal of time investigating spiritistic phenomena. The year 1869 saw the beginning of the cooperation between himself and Myers, which was largely instrumental in founding the S.P.R. in 1882. In his address, "In Memory of Henry Sidgwick," Myers wrote of the occasion (December 3, 1869), "I asked [Sidgwick] almost with trembling whether he thought that when Tradition, Intuition, Metaphysic, had failed to solve the riddle of the Universe, there was still a chance that from any observable phenomena—ghosts, spirits, whatsoever there might be—some valid knowledge might be drawn as to a World Unseen. Already, it seemed he had thought this possible; steadily though in no sanguine fashion, he indicated some last grounds of hope; and from that night onward I resolved to pursue this quest, if it might

be, at his side."[2] The quest upon which these two friends set out together was to play a large part in the foundation and early work of the S.P.R., and for this purpose their differing personalities formed an admirable blend; Myers' sanguine and emotional temperament (the temperament of a poet) needed the guidance of Sidgwick's cooler judgment.

Sidgwick's death in 1900 after a few months' illness was a heavy blow. I have recorded in the Myers Memorial Lecture, which I delivered in 1945, my vivid recollection of the last time I saw him: "He passed me one day walking up the road toward his home in Newnham College, and something in the brooding calm of his face struck my imagination. When a few weeks later my mother told me of the fatal illness from which he soon afterwards died, I remember thinking to myself, 'He must have known what was before him that day I passed him in the road.'" I also remember hearing from my mother, who was present on the occasion, the deep impression made on the company at a lunch party held at Myers' house, Leckhampton, a few days after Sidgwick had heard from the doctors that he must immediately undergo a serious operation, and was not likely to live long. The incident is recorded in the Memoir of Henry Sidgwick: "The last of many lessons I learnt from him, the most beautiful and the most unforgetable, was at the lunch at Leckhampton on May 27. He taught me there how calmly and manfully death and suffering could be faced, as he recited without a break in his voice the lines which I could hardly bear to hear, from 'Super Flumina Babylonis'[3] ending

> Where the light of the life of him is on all past
> things, Death only dies.

I think that the sound of his voice and the light on his face will be before me when the call comes for me; and I shall be grateful then for his death as well as for his life."[4]

With Myers' interest in psychical research I was not during his lifetime much concerned. I took part with my mother in some early thought-transference experiments, but I regarded them as a kind of game. But perhaps my recollections of the man himself and my

[2] *Proc. Soc. psych. Res.*, London, **15**, p. 454.
[3] *Swinburne.*
[4] *Henry Sidgwick: A Memoir* by Arthur Sidgwick and Eleanor M. Sidgwick. London: Macmillan, 1906. P. 588.

knowledge of what he meant to some of his friends and fellow-workers have helped to give me a more vivid idea of his personal contribution to psychical research.

Myers, like Sidgwick, was a Fellow of Trinity College, but he did not take up an academic career. His early tastes were classical and literary, and by profession he was an Inspector of Schools. He has himself given an account of his early years and the influences which went to mold his character in his "Fragments of Inner Life." My own youthful impressions of him are of a vivid and occasionally formidable personality. I well remember the day when I unwittingly incurred his displeasure by tramping heavily across a field of virgin snow which his poetic eye had wished to see unsullied. I was left in no doubt of my offense. But these flashes of anger were short-lived (one became, as children do, philosophical about them), and I remember many other occasions when Myers' lively humor and enthusiasm made him a delightful companion.

He had a very wide knowledge of verse, serious and comic, and would often break into a dramatic recitation of some favorite passage. I can hear his voice now sonorously rolling out lines from his favorite, Virgil; and I remember one day he gave an impromptu and impressive recitation of Browning's poem "Love Among the Ruins." His dramatic talents were considerable, and there were happy occasions when he was persuaded to take part in our youthful charades, or in some scene or play we were "getting up."

The Myers' had a beautiful house and garden on the outskirts of Cambridge where they entertained many guests, including some of the celebrities of the day. The African explorer, Henry Stanley, was often there (he married Mrs. Myers' sister), and I remember our excitement as children on hearing that Henry Irving was expected, and the thrill of seeing the great man himself walking on the terrace. Myers' social gifts and connections were not without their value in the days when to admit an interest in psychical research was regarded in many quarters as the credulous vagary of a few cranks.

There were also other visitors to Leckhampton more closely connected with psychical research, either as mediums, or investigators. When the Cambridge sittings with Eusapia Paladino were being held, she was for several weeks a guest in the house. She was

an almost illiterate peasant woman, ill-tempered and egotistic, and it was often difficult to keep her entertained. We children were called in to take part in this task by playing games with her, and it was impressed upon us that we must not thwart Eusapia, but must let her win when she chose, so that she might be in a good temper for the séance to be held in the evening. All properly minded children have a strict code of honor where games are concerned: Eusapia had no code of honor at all, and her company was sometimes a severe trial to us. Henry Sidgwick, to whom, though we did not realize it at the time, I have since thought her company must have been an even greater trial than it was to us, was wonderfully patient with her. There is a delightful photograph of the two together, Eusapia in cap and gown, as proud as a peacock, and Sidgwick looking at her with a kindly smile in which there is just the faintest possible trace of philosophic doubt.

Richard Hodgson, who came over from America to take part in the sittings and was largely instrumental in detecting the method of fraud Eusapia practiced at them, was a companion much more to our liking. Wherever he went, Hodgson made friends with children.

In the case of Myers, as with both the Sidgwicks, it hardly seems necessary to say much of the intellectual debt which psychical research owes him: it is universally recognized by all concerned. But there are one or two points I would like to emphasize. Myers did great service in the early days by his talks and lectures. He was not only brilliant, but extraordinarily ready; he could speak almost without preparation to any audience on any branch of the subject, and *hold* them—a rare gift, and most valuable when publicity and the "preaching of the gospel" are required. The only psychical researcher in England at all comparable to Myers was Sir Oliver Lodge.

The other point I have in mind which has added to the durability of Myers' work is his faculty—one might almost say his genius—for seeing whither certain lines of contemporary thought were likely to lead and which of them would be fruitful. He was one of the first people to recognize the importance of Freud's work when Freud was an almost unknown man and by no means well thought of by orthodox psychologists. Sir Oliver Lodge, writing

"In Memory of F. W. H. Myers," said: "Myers spoke to me once of the possibility that parts of an atom move perhaps inside the atom in astronomical orbits, as the planets move in the solar system, each spaced out far away from others and not colliding, but all together constituting the single group or system we call the atom—a microcosm akin to the visible cosmos, which again might be only an atom of some larger whole. I was at that time disposed to demur. I should not demur now."[5]

This prescience arose no doubt from Myers' exceptional powers of assimilating knowledge (Gurney said of him: "Whilst I am reading a book, Myers will master a literature."), combined with great powers of coordination. His mind ranged freely and retentively over every field from which he might glean facts of value, and then what he would have called his subliminal self pieced these gleanings together into the shape of things to come.

In concluding what I have to say of Myers I will return to the personal aspect of his work. Myers, as he himself has told us, was a man in whose life personal emotion played a compelling part. The intense ardor with which he threw himself into the quest for evidence of human survival was heightened by, though it did not originate in, a personal experience known to some of his friends, of which traces may be found in his published poems. The intensity of Myers' ardor aroused a corresponding ardor and the will to work in others; it may have aroused not only the will to work but the ability to produce results, providing that *tertium quid*, of which Professor Gardner Murphy spoke in his recent Presidential Address to our Society, "personality as a node or region of relative concentration in a field of vast and complex interpenetrating forces." One need not beg the question of the possibility of communication between the living and the dead to understand how Myers' passionate desire to find scientific proof of survival may have acted not only during his lifetime, but afterwards, in the impulse of his friends and fellow-workers to further in any way possible the cause he had so much at heart, an impulse which undoubtedly played a considerable part in the long series of experiments in automatic writing published in S.P.R. *Proceedings* from 1904 onward.

[5] *Proc. Soc. psych. Res.*, **17**, p. 7.

The only one of the S.P.R. founders of whom I can claim to have been a colleague is Mrs. Sidgwick. My earliest recollections of her, which are more than childish memories, date from my college days, when she was Principal of Newnham College. To the students of my day she was an important, but rather remote, figure; we regarded her with great admiration, and in a way with affection too, for she seemed to embody all that we could wish to see embodied in the Head of our College. But few of us really knew her. Our personal contacts were mainly when we were invited to dine with her at the "high table," perhaps once or twice a term. She was not a person with whom the young found conversation easy. It has often been remarked that Mrs. Sidgwick had no small talk, no conversational gambits. I have thought since that my friends and I would have got on better if we had shown more initiative and chosen a subject for conversation according to our own tastes. But young people are apt to leave the initiative to their elders, and there would sometimes be long silences. I learned in later days that there was no need to feel embarrassment at being silent in Mrs. Sidgwick's company; if one had nothing particular to say, one might just as well (or better, indeed) not say it. But I do remember in my Newnham days wishing sometimes that Mrs. Sidgwick would begin!

I first got to know her as a psychical researcher when I began to work for the Society in 1908, and I was associated with her in the Society's work until within a short time of her death. I have no hesitation in saying that, taking character and intellect together, Mrs. Sidgwick was one of the most remarkable people (men or women) that I have ever known. Of her intellectual attainments I need not write; they have been recorded in many fields. But her character was equally outstanding, and it reinforced her intellect. Two things which seem to stand out are her simplicity and her complete lack of that egotism and tendency to self-importance by which most of us are more or less influenced. I cannot recall any occasion when Mrs. Sidgwick ever made any claim for herself beyond what she might reasonably claim as a means of furthering the task upon which she was engaged. Nor do I ever remember her refusing any task she felt she could usefully perform on the ground that it was trivial or of a subordinate kind. She invariably, for instance, corrected her own proofs at all stages with the "copy,"

and she was an admirable proofreader. Proofreading, as I can testify from personal experience and observation, is a great test of accuracy and continuous concentration of mind. Mrs. Sidgwick was a faultless reader, and I would not care to say as much for any other contributor to our S.P.R. *Proceedings* who has come under my editorial eye.

It was only natural that the high standard of scientific accuracy and complete moral and intellectual integrity which Mrs. Sidgwick herself maintained should make her critical of shortcomings in others, and on occasions she had, no doubt, a chilling effect. I remember one such occasion when I was staying at Fishers Hill (where Mrs. Sidgwick was then living in the home of her brother, Lord Balfour), one of my fellow-guests being Miss Alice Johnson. Dame Ethel Smyth, the well-known musician, brought Miss Oenone Somerville, author of "The Experiences of an Irish R.M.," to the house to relate to Mrs. Sidgwick and Miss Johnson a remarkable story of the finding of a leprechaun's shoe on an Irish moor. When Miss Somerville was asked how the interview had gone, she replied: "Not at all well. Those two white-headed old weevils would bore a hole in anything."

I recall another occasion when the strictly scientific attitude of the S.P.R., as exemplified by Mrs. Sidgwick and Miss Johnson, was found uncongenial to the celtic temperament. At that time the Society had rooms in Hanover Square, and on the same floor the Irish Literary Society had a room. The Irish poet, W. B. Yates, who was for some years a member of the S.P.R., often came to see his literary friends, and sometimes he would drop in and have a chat with me next door, telling me some of the psychic experiences he and his friend, AE, had had together. Complaining of the cold atmosphere in which I seemed to be enveloped, he said once, "It's my belief that if you psychical researchers had been about when God Almighty was creating the world, He couldn't have done the job."

But for all her stringent demands in the matter of control and evidence Mrs. Sidgwick's kindness and simplicity enabled her to establish friendly personal relations with most of those whose phenomena or experiences she had to investigate. I remember, for instance, that when Mrs. Sidgwick and I had sittings together with the Polish medium, Stanislawa Tomcyk, the childish secondary per-

sonality who emerged in trance liked Mrs. Sidgwick better than any of us. She was also a favorite with Mrs. Leonard's "Feda." I think these sittings with Stasia Tomcyk were the last sittings for physical phenomena in which Mrs. Sidgwick took part. With her usual common sense she realized that her own diminishing powers of perception, both of sight and hearing, were beginning to make her a less efficient observer; but it is sometimes forgotten now what an immense amount of time Mrs. Sidgwick devoted in the early days of the Society to the investigation of physical mediumship. On one occasion at a meeting where there had been a long and rather fruitless argument about physical phenomena, in which Mrs. Sidgwick had taken no part, her voice was heard observing quietly, "I suppose I have sat with more physical mediums than any of you," and that remark was doubtless true.

Another of Mrs. Sidgwick's characteristics which I call to mind was her extraordinary indifference, even in old age, to what most of us would regard as hardships. Such words as "Spartan" or "austere" would not be appropriate because they suggest a deliberate rejection of comfort and good things which was quite foreign to her character. She just didn't seem to be aware of any hardship. She would come to the Society's rooms in Tavistock Square from her home near Woking, and late in a winter's afternoon (perhaps a foggy afternoon) she would go cheerfully off to catch the bus for Waterloo Station, steadfastly refusing to have a taxi called. On one memorable occasion, when the frost was so hard that all the pipes in Tavistock Square had burst and the house was festooned with icicles, the Secretary, Miss Newton, was astonished to see an open car drive up to the door, and in it Mrs. Sidgwick and her sister, Lady Rayleigh, both of them over eighty years of age. When Miss Newton remonstrated, the reply she got was, "My sister always drives in an open car; she likes the air, and I don't mind."

She had a superb physical constitution with which it was possible to take these liberties, and she kept both her physical and mental energies almost to the end of her long life. She was eighty-seven when she wrote her last contribution to the S.P.R. *Proceedings,* delivered by her brother, Lord Balfour, on the occasion of the Society's Jubilee in 1932. It was at this meeting that Lord Balfour was authorized to convey to the audience Mrs. Sidgwick's assurance

that "upon the evidence before her, she herself is a firm believer both in survival and in the reality of communication between the living and the dead." This assurance in Mrs. Sidgwick's case was a slow growth, based on a lifetime of careful and singularly unbiased sifting of evidence, and whether we happen to share Mrs. Sidgwick's assurance on this point or not, we must admit that few people have had a better right to feel assured.

I have written above of the sittings I had with Stasia Tomcyk (I have always regretted that these sittings were cut short by the 1914 war, for they were by far the most interesting sittings for physical phenomena in which I ever took part), and this brings to my mind pleasant recollections of Everard Feilding, for many years one of the Society's Honorary Secretaries. His own interest was mainly in physical phenomena, and his report on the Naples sittings with Eusapia Paladino offers the only evidence for the genuineness of her phenomena to which it seems likely the future student will attach much value. He took a keen and sometimes impish delight in observing and exposing the odd vagaries of the fraudulent medium, and however disappointing the phenomena might be, he could always find some pleasure in the psychological twists of mediumship. I remember one occasion when I went to a sitting at his house with two foreign mediums (husband and wife) who claimed to produce apports. Searching the room in which the sitting was later to be held, Mr. Feilding found several dead prawns concealed in a hanging electric light shade, and evidently destined to appear as apports. He said nothing of his discovery to the mediums, or anyone else, but he tied bows of pink ribbon round the prawns' necks. The feelings of the mediums can easily be imagined when in due course the "apports" complete with ribbons fell upon the séance table. The mediums left next day and no further sittings were held. But in spite of this and many other disappointments Feilding maintained to the end that he had witnessed some genuine phenomena.

Alice Johnson, under whom I served my apprenticeship in psychical research, was in various capacities a salaried officer of the S.P.R. from 1899 to 1916. Her training was scientific; she was a student of Newnham College, took a first class in the Natural Sciences Tripos, and was for several years a Demonstrator in Animal Morphology (at Newnham). She became interested in psychi-

cal research mainly through her associations with the Sidgwicks and from 1889 onward took an active part in the work. She was a member of the committee which collected evidence and presented the Report on the Census of Hallucinations (1894). When Frederic Myers died leaving his great work, *Human Personality,* uncompleted, it was to Alice Johnson in cooperation with Richard Hodgson that was given the task of preparing the work for press. She contributed many important papers to the S.P.R. *Proceedings* and played a leading part in the study of the "S.P.R. group of automatists" and the development of cross-correspondences.

But much of Miss Johnson's work was done behind the scenes and was of a kind that has no written record. She first began to play an important part in the practical direction of S.P.R. affairs at about the time the Society was feeling the double loss inflicted by the deaths of Sidgwick and Myers. It was in a considerable measure owing to her influence, constantly exerted in the day-to-day control of policy, that the Society's scientific standards were maintained. She was a shy woman, of frail, and at first superficial glance, unimpressive appearance; but she had exceptional strength of character. She was by family tradition a nonconformist with a strain of the old Puritan in her make-up. She would have gone to prison, or even to the stake willingly—had she lived in the days when such heroic sacrifices were demanded—for any cause she deemed good; and in the forefront of good causes she put psychical research. She stood like a rock against any attempt to lower the standards by which she considered the Society must stand or fall.

When I first began to do regular work for the S.P.R. in 1908, I was appointed as Miss Johnson's assistant. I was set at first to simple tasks of one sort or another, bottle-washing, one might say, and gradually as I learned my job I was given more independence and responsibility. Looking back on those days, I can see how good Miss Johnson's methods of training were. The work we had to do then was mainly qualitative, not quantitative, as so much psychical research is now; but there was just the same need of accuracy and perhaps an even greater need of good judgment. Upon scrupulous accuracy Miss Johnson always insisted, and her judgment was excellent.

She was chary of praise, and I used to think in early days that her criticisms of my work were rather meticulous (I do not think so now); but she was always fair and ready to give good work its due. She was also unfailingly kind and courteous, both to myself and to other members of her staff. In temperament, for all her shy, retiring manner, she was something of an autocrat. She knew clearly in her own mind what needed to be done and was resolved to do it. She did not suffer fools gladly; she would be outwardly courteous, even to a fool, but to those who knew her there would be subtle indications of her inward thoughts.

She was a good judge of character (a matter of some importance in psychical research) and in spite of her critical outlook and natural reserve, she had wide human sympathies. She was able to get on friendly terms with all sorts of people, provided she felt them to be genuine: anything in the way of pretentiousness, humbug, or sentimentality she was quick to resent. She had a pleasant sense of humor, and if a letter or interview had a comic side, she could be trusted to see it. I used to find excellent entertainment in the interviews I sometimes witnessed between Alice Johnson and Everard Feilding. She did not altogether approve of Mr. Feilding; she did not approve of his light-hearted approach to a serious subject, nor of the impartial zest with which he pursued, and even encouraged, fraudulent phenomena for the interest of observing them and studying the psychology of their perpetrators. She would take him vigorously to task. But Mr. Feilding could be as pertinacious as Miss Johnson in the pursuit of his own ends, and he had much natural charm. Sooner or later he would find a joint in Miss Johnson's armor, and the interview would end in friendly laughter and a drawn game. As soon as he had gone, Miss Johnson would become severe again. "Mr. Feilding," she would say, "is hopeless."

One of the points upon which these two would agree to differ was in regard to the attitude that should be adopted toward mediums detected in deliberate fraud. Miss Johnson shared the view held by Mrs. Sidgwick and some others, but not accepted at the time of which I am writing by the S.P.R. Council as a whole, that the Society should refuse to investigate any such medium.

Alice Johnson's own interest was mainly in the mental side of psychical research, and especially in automatic writing. As I have

said, the years during which she was most closely associated with the Society's work covered the period of the cross-correspondences, and her fearless originality of mind was of much value in doing pioneer work with a new type of material. The peculiar combination of qualities which this work required afforded justification for her view that psychical research is a subject *sui generis,* needing special training and experience. She wrote in the S.P.R. *Journal:* "If a supposed discovery is made in chemistry, its acceptance by the scientific world does not depend on whether the discoverer is, say, an expert astronomer, but on whether he is a competent chemist. But if a supposed discovery is made in psychical research it is almost invariably recommended to the world on the ground that the discoverer is an eminent physiologist, or a distinguished psychologist, or astronomer, or physicist, whereas it appears to me that the only relevant consideration is his competence as a psychical researcher." Miss Johnson's competence in her own chosen field no one who worked with her is likely to doubt.

In conclusion: when I cast my thoughts back to this group of workers, of whom I have tried to give some memories and personal impressions, what stands out in my mind as something common to them all is the tireless, ungrudging enthusiasm with which they gave themselves to their work. They were determined to establish psychical research as a proper subject for scientific inquiry, and in that purpose they may fairly be said to have succeeded. It is in the same spirit which animated them that further advances will be made.

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# DISPLACEMENT IN ESP CARD TESTS IN RELATION TO HITS AND MISSES

By J. G. Pratt and Esther Bond Foster

**ABSTRACT: In this investigation of a series of ESP card tests done by a single subject, attention has been focused upon the subject's calls that missed the card targets for which they were intended. These wrong calls were checked against targets coming both before and after them in the card series and the results gave significant evidence that ESP had "displaced" or spread to these neighboring card positions. However, the way in which the wrong calls were related to the preceding and following targets (whether the coincidences were above or below "chance") was found to depend on whether or not those targets had been hit or missed by their own calls. Thus the spread of ESP to adjacent cards was of two distinct patterns, one for hits and one for misses. The two main findings that have come out of the investigation are as follows:**

**(1) Targets which were missed by the calls intended for them were also missed significantly more often by the calls which preceded them than by the calls which followed them—when these adjacent calls also missed their own targets. This difference was found to be chiefly due to the fact that these preceding calls hit below chance expectation on the targets one place ahead.**

**(2) The results for wrong calls before and after hits on the intended targets was just the reverse of that found for wrong calls coming before and after misses. That is, targets which were hit by the calls intended for them were hit more often by the preceding calls than by the following calls when these preceding and following calls missed their own targets. It is not known at this time whether this difference is chiefly due to anticipation of the hits before the subject got to them or to avoidance of the hits after she was past them.**

**Dr. Pratt is a regular contributor to the Journal. His collaborator, Mrs. Foster, is a research assistant in the Parapsychology Laboratory and the author of a research report in the 1937 Journal under the name of Esther May Bond.—Ed.**

## Introduction

The December, 1949, issue of the Journal carried a report (2) of an investigation of the relative frequency with which individual subjects repeated a call symbol in ESP card tests on the trial following a hit and on the trial following a miss. The records examined were all from experiments in which the subjects were not told whether a call was a hit or a miss until the end of the run. The question in mind at the time the investigation was started was one that had been suggested earlier by Soal (4): If there is a difference in the relative frequency of repeating a call after a hit and

after a miss, does this fact indicate that the subject is reacting differentially to ESP success and failure?

Two subjects were discovered, both of them from the University of Colorado experiments of Martin and Stribic (1), who showed a statistically significant tendency to repeat their call symbols more frequently after misses than after hits. A third series, in which blind children were used as subjects (3), showed a significant tendency to do the opposite; that is, to repeat call symbols more frequently after hits than after misses.[1] The question of whether the results indicated a direct reaction to ESP success and failure, rather than being simply a peculiarity in the sequence of responses connected in some way with hits and misses, had to be left open.

We have continued the investigation with the aim of throwing further light upon the meaning of the previous findings. As a first step beyond the point reached in the December report, a more detailed study of the data of the two Colorado subjects, D.W. and C.J., was made. Also, the investigation has now been extended to include data that were not available at the time of the earlier study. These efforts have revealed other highly significant relationships in that part of the ESP data of D.W. and C.J. that was covered in the earlier report. The findings have already been confirmed in other parts of the extensive University of Colorado records[2] as well as in the data of other ESP experimenters on file in the Parapsychology Laboratory.

While these more recent findings are not at variance with those reported before, they go beyond the point reached in the previous paper. The results suggest that ESP was influencing a much larger percentage of the calls in these records than is indicated by the number of hits.

The present paper will present only the findings that have emerged from a more detailed study of the D.W. series. These

[1] Whenever the simple terms "hit" and "miss" are used in this paper without any further qualification, they will always mean that the symbol called by the subject either matched or differed from the target for which it was intended (that is, the "assigned" target). Later in the paper it will be necessary to show this same relationship more precisely. When this is the case, a call symbol which matches its assigned target will be referred to as a (0) hit; a call symbol which does not match its assigned target will be a (0) miss; and the assigned target itself will be referred to as the (0) target.

[2] We wish to express our appreciation to Dr. Dorothy L. Martin and Dr. Frances P. Stribic for making their data available for this investigation.

results will serve to illustrate the direction the investigation has taken and the kind of results that are being obtained. This account is offered as an interim report of work in progress. The chief reason for publishing it at this time is to get the widest possible discussion of the methods of analysis and of the interpretation of the results. We are aware that the use of special groupings of ESP data in relation to hits and misses raises statistical issues that have an important bearing upon the question of how to make an exact assessment of the findings. However, the line of investigation seems worth while even with the difficulties as they appear at present, and it is reasonable to expect that they can be removed or surmounted.

## General Results

The D.W. series from the University of Colorado experiments consists of 301 runs through the standard ESP deck of 25 cards which contains five each of the five different ESP symbols. The experiment was a test of clairvoyance, and the DT or "down through" condition was used throughout the series. The subject's calls made an average of 7.50 hits per run on the assigned targets, the cards at which she was consciously aiming her calls. This is a highly significant rate of success, as her total deviation from mean chance expectation was +753. A score equal to or greater than this would be expected as a chance occurrence an average of only one time in more than $10^{95}$ repetitions of the experiment.

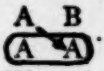
In the course of the investigation reported in the previous article, the D.W. records were examined for a possible relationship between the subject's call and the target of the preceding trial; that is, for evidence of (−1) backward displacement. The reason for doing this was stated in the preceding article, and it is sufficient only to mention it here. It was possible that a significantly low score on backward displacement occurred in the D.W. series as a general effect, one having no specific relation to hits and misses on the assigned targets. If it did, this would have produced as an artifact the appearance of changing the call symbol after a hit. But if a low score on backward displacement in the data as a whole was the explanation of the failure to repeat the call after a hit, the calls following misses should show the same degree of avoidance of the

backward displacement targets as did those following hits. A check for backward displacement on the calls following (0) misses showed that the (−1) score made by these was very close to the mean chance expectation (M.C.E.). *Thus there was no general tendency to avoid the (−1) targets.* This meant that the tendency to change the call symbol following hits could not be explained on the ground that calls *in general* avoided the preceding targets.

The study of the sequence of calling in relation to hits was carried one step further in the previous paper to check upon whether the peculiarities in the call sequence following hits could be explained by the subject's tendency to call a large number of doubles. This was done by observing whether an incorrect call coming before a hit made a *double* with the correct call more often than did an incorrect call following a hit. These relationships can be understood more clearly from a diagram.[3] It was found that patterns of calls and cards of the type were to a statistically significant degree more frequent in occurrence than patterns of the type . As matters stood at the time that finding was reported, however, it was not known whether the observed difference was attributable to an excess of doubles formed by the incorrect calls that preceded hits (first diagram) or to a deficit of doubles formed by incorrect calls following hits (second diagram).

A possible explanation of this finding is that the larger number of doubles formed by the calls coming before hits reflected a general tendency for the subject to make a large number of hits on (+1) forward displacement. In other words, D.W. might have scored significantly above M.C.E. on forward displacement when

[3] In the schematic representations of calls and cards that appear in this paper, the letters A, B, C, D, and E are used instead of ESP symbols for convenience of printing. Calls are always shown on the left and cards on the right; and hits on the assigned targets are circled. A diagonal line will be used to show what direction of displacement the diagram is primarily intended to illustrate. Thus indicates that symbol A was first called and that it was a miss on its assigned target, B; that the second call, C, was a hit on its assigned target, C; and that call A was a miss on the (+1) forward displacement target, C. Similarly (−1) backward displacement is shown in this way: . (When the displacement, as in this case, is a hit, the diagonal line is provided with an arrow point.)

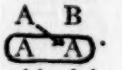
no (0) hits were involved, such as , as well as when the second call was a (0) hit, as in  . To test this possible explanation, all the data of the D.W. series were checked for (+1) displacement, and the resulting score was evaluated in a way that took account of the fact that the closed deck was used. The overall forward displacement score was found to be *significantly below M.C.E.*[4]

Here was a curious finding indeed! We had been led to examine the total (+1) displacement in the first place because the type situations were significantly more numerous than the type, and we wished to see whether a positive deviation on forward displacement in the data as a whole could explain this difference. Yet the total score on forward displacement turned out to be significantly below M.C.E. This suggested that at least two distinct and opposed trends of ESP displacement were taking place in these data. Around targets on which (0) hits were made, forward displacement was significantly higher than backward displacement. But since forward displacement as a whole was significantly below M.C.E. it must have been lower than backward displacement around targets on which (0) misses were made.

As a first step toward checking on this suggestion, we examined all the pairs of consecutive misses for forward and backward displacement. Three misses in succession were treated as two pairs, four misses as three pairs, and so on. No carry-over was made from the end of one run to the beginning of the next. A total of

[4] As this finding was important chiefly as a spur to further efforts to sort out the displacement peculiarities in the D.W. data, it will suffice merely to indicate the general results of the analysis. There are always a fixed number of hits, 125, that can be made in a closed deck when each call in the run is checked against every card in the target series. The fact that D.W. scored so far above expectation on the assigned targets means that, on the average, a low score is expected on displacement. The CR of the total (+1) forward displacement was —3.3 (P = .001). It may be noted that while this result was not independent of the subject's tendency to call doubles, the observed doubles in the calls would have had the effect of bringing the negative deviation on forward displacement closer to M.C.E. The preceding paper indicated that in evaluating evidence for displacement it is necessary to take into account a subject's tendency to call doubles. The present report will further emphasize the importance of this point.

3,545 such pairs were found. The (+1) score—the number of times the first call matched the second card: A/C B/A—was 555 hits. The (−1) score—the number of times the second call matched the first card: A/B B/C—was 691 hits. Within pairs of misses, therefore, the forward displacement score was 136 *smaller* than the backward displacement score.

On the other hand, as stated above, the relation between the two displacement scores is reversed when a call that missed its assigned target is checked against a card one step removed that was hit by its own call. As shown in the previous report, there were 1,548 pairs of trials consisting of a miss followed by a hit in this series, and 415 gave forward displacement hits, as in  . On the other hand, there were 1,530 pairs consisting of a hit followed by a miss, and only 332 of these gave backward displacement hits,[6] as in . Thus when the displacement target was one that had been hit by the call intended for it but the displacing call was one that had missed its assigned target, the (+1) score was 83 *larger* than the (−1) score.

To summarize briefly: I. When displacement within a pair of trials consisting of two (0) misses was considered, the (+1) score was 136 *smaller* than the backward displacement score. II. However, when the displacement target was one that was hit by its own call and the displacing call was a miss on its assigned target, the forward displacement score was 83 *larger* than the backward displacement score. These two displacement situations will be referred to hereinafter as Type I (misses into misses) and Type II (misses into hits), respectively.

How may these differences properly be evaluated to determine whether they are beyond the range of reasonable chance variations? When the ESP targets of a high-scoring subject are separated into two classes, the hits in one group and the misses in another, it may not be assumed that these two subclasses are strictly random sam-

[6] This number was cited as 334 in the preceding paper, but errors found in further checking reduced it to 332.

ples drawn from the original card population. For example, closed decks were used in the D.W. tests, and it is certain that the subject very rarely hit an equal number of the five different target symbols in each run. The targets that were hit would therefore comprise an uneven distribution of symbols, and the missed targets would do so also, since the number of misses for each symbol is the difference between five and the number of hits made on each target. However, when we considered the call symbols that missed the targets at which they were aimed, we found that the wrong call symbols were about equally likely to be associated with each one of the four target symbols that were not like the incorrect call symbols. This is a sufficient condition for the (+1) and (−1) displacement expectations to be equal in the pairs of misses (Type I).

Although the expectations for (+1) and (−1) displacement within the pairs of misses are approximately equal, these scores are not completely independent of each other. Nor may it be assumed that in the Type II situation (misses into hits) the scores from (+1) and (−1) displacements are independent of each other. In the previous paper on change of call, the data for the latter case were evaluated by means of a chi-square test. Dr. T.N.E. Greville pointed out after that report was published that the chi-square test assumed independence of the categories involved and that there might therefore be grounds for questioning its appropriateness in this instance. He suggested that, since the expectations are equal for (+1) and (−1) Type I displacement (misses into misses), a *t*-test might be applied to the distribution of the 301 observed differences between them, one difference for each run, to see whether the mean of the observed differences was significantly different from the expected mean of zero.

To get the data for the *t*-test, the difference between (+1) and (−1) displacement was observed for all the pairs of misses in each run. If the (−1) score for the run was higher, the difference was recorded as a positive number; if the (+1) score was higher, as a negative number. The *t*-test was then applied to the 301 run differences in displacement within the pairs of misses. This showed that, for Type I (misses into misses) displacement, the (−1) "run" score average was .452 higher than the (+1) average. This difference based on the 301 runs gave a $t$ of 4.16 ($P = .00003$).

This is a highly significant result which supports the conclusion that the difference between (+1) and (−1) in the pairs of misses did not arise fortuitously. On the assumption of equal expectations, the *t*-test was also applied to the (+1) and (−1) differences for the Type II situations based upon the sequence, miss-hit-miss, on three successive trials. (This assumption of equal expectations seems to be reasonable for a subject who calls about a chance number of doubles.) These cases in which a single hit was both preceded and followed by a miss always involved the same target symbol for Type II (misses into hits) forward and backward displacement. They were selected for this analysis because they presented a situation that was not complicated by two or more hits in succession. The first call of the miss-hit-miss sequence was checked for forward displacement into the middle target, which was hit by its own call; and, contrariwise, the third call was checked for backward displacement. The difference between (+1) and (−1) displacement for the 3-trial sequences was obtained for each of the 301 runs, and the distribution was evaluated to see whether its mean differed significantly from the expected mean of zero. The (+1) run score average in these selected 3-trial sequences was .249 higher than the (−1) average. This average difference, based on 301 observations, gave a *t* of 3.13 (P = .0013). Thus, the (+1) score is significantly higher than the (−1) score when only the selected case of the miss-hit-miss sequence is considered.[6]

Of course, Type I (misses into misses) and Type II (misses into hits) do not account for the total displacement as it is generally checked for the run as a whole. Two other cases of displacement as it is related to hits and misses may be designated. Type III (hits into misses): A call that hits its assigned target also makes a displacement hit on a target missed by its own call, such as

for (+1) and

for (−1). Type IV (hits into hits): Two

[6] This evaluation is based upon only a part of the D.W. data that were evaluated by the chi-square method in Table 6 of the previous paper on change of call. The cases excluded are the misses which are separated by two or more hits; the backward displacement of the first call that missed its assigned target in a run that begins with a hit; and the forward displacement of the last (0) miss in a run that ends with a (0) hit. The *t*-test of the difference between (+1) and (−1) displacement for Type II (misses into hits) for the special miss-hit-miss case gives the same degree of statistical significance as the previous chi-square test.

successive (0) hits are made on the same target symbol, and therefore each call makes a displacement hit on the other's card, as . These types of displacement are closely linked with scoring on the assigned target. As the evidence for ESP based upon D.W.'s number of (0) hits is so much stronger than that based upon her displacement peculiarities, it seems better to reserve judgment about the interpretation of displacement results found from Type III and Type IV cases. If points of special interest regarding them are found, it will be necessary to look first for the explanation in terms of the high level of scoring on the assigned targets. However, to complete the picture, they have been checked and the results for all types of displacement are shown in Table 1.

The total (−1) score of all the D.W. data is 96 larger than the total (+1) score. This overall difference is of only suggestive significance, the CR being 2.0 (P = .05, approximately). It is evident, therefore, that there is a considerable cancellation of the differences when the four different types of displacement are pooled.

The investigation was extended to include the comparison of calls with the targets that were two and three places away. The data on all the displacement categories checked are shown in Table 1.

Table 1

TOTAL DISPLACEMENT RESULTS

| | Type I Displacement (misses into misses) | Type II Displacement (misses into hits) | Type III Displacement (hits into misses) | Type IV Displacement (hits into hits) | Total Displacement Score |
|---|---|---|---|---|---|
| (+1)..... | 555 | 415 | 204 | 127 | 1301 |
| (−1)..... | 691 | 332 | 247 | 130 | 1400 |
| Diff..... | −136 | +83 | −43 | −3 | |
| (+2)..... | 837 | 144 | 233 | 55 | 1269 |
| (−2)..... | 880 | 99 | 272 | 55 | 1306 |
| Diff..... | − 43 | +45 | −39 | 0 | |
| (+3)..... | 819 | 153 | 273 | 67 | 1312 |
| (−3)..... | 795 | 146 | 282 | 67 | 1290 |
| Diff..... | + 24 | + 7 | − 9 | 0 | |

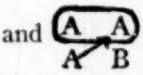
These wider displacement spans on the whole did not give statistically significant results for the D.W. series, but the difference be-

tween (+2) and (−2) for Type II displacement is strongly suggestive even when allowance is made for the selection of the best result.[7] Also, they follow the general pattern of the findings for (+1) and (−1): the (+2) score is *lower* than the (−2) score for both Type I and Type III displacement; and the reverse is true for Type II. By the method of estimation used, the (+2) and (−2) difference for Type I has a probability of .3, and that for Type II, .004.

## Discussion

The above analysis serves to show that there is a reliable difference between (+1) and (−1) for both Type I displacement (misses into misses) and Type II displacement (misses into hits). When pairs of misses alone (Type I) are considered, the (−1) score is significantly higher than the (+1) score. When Type II displacement (misses into hits) is considered, the difference is reversed, and the (+1) score is significantly higher than the (−1) score. Naturally when these two opposed displacement trends are added together, as they are when displacement is checked in the run as a whole, they tend to cancel each other.

The establishment of these significant differences between forward and backward displacement of Type I (misses into misses) and of Type II (misses into hits) leads to the important question: Which displacement, (+1) or (−1), is responsible for the observed differences? Are the differences due to deviations by either the (+1) score or the (−1) score alone, or does the expected score lie midway between the observed scores?

The problem of finding the expected displacement scores for the Type II situation (misses into hits) is complicated by the fact that displacement hits depend in this case upon doubles in the calls, such as for (+1) and  for (−1). If a subject never calls a double, his Type II displacement score would be zero; if he should call the same symbol all the time, he would get a perfect score on

[7] The significance of the differences between forward and backward displacement for (+2) and (−2) was tested by a method of approximating their occurrence as chance events. The method used was as follows: $\chi^2 = \frac{(x-y)^2}{x+y}$ where $x$ and $y$ are the two displacement scores concerned and $\chi^2$ has one degree of freedom.

Type II displacement. At the present time, we see no way of arriving at an expected score for Type II displacement as a factor independent of the doubling tendency. This does not mean, of course, that the difference between (+1) and (−1) in Type II (misses into hits) is unimportant from a psychological point of view, but only that it is ambiguous at this time as far as ESP displacement as such is concerned.

On the other hand, displacement in the Type I situation (misses into misses) is only partly dependent upon the subject's doubling tendency, and it is possible to consider what score is expected from displacement as an independent factor. We have approximated the displacement expectations in the pairs of misses in several ways, and all the methods yield values that lie close to the score actually obtained from backward displacement. The expectations calculated by the different methods range from 650 to 680. If the true value lies between these figures, the deviation for (+1) displacement would be between −95 and −125, while that for (−1) would be between +11 and +41. The indications are, therefore, that the difference between (+1) and (−1) in Type I is chiefly due to a negative deviation on (+1). A method for making an approximate evaluation of one direction of Type I displacement by itself is given in the Appendix.

The question arises whether the differences between (+1) and (−1) scores for types of displacement I, II, and III are accounted for equally by the five call symbols. When the differences were examined symbol by symbol, it was found that there was a difference between the strength of the displacement effects associated with the circle and wave calls on the one hand and the square, star, and cross calls on the other. The circles and waves were the two symbols which D.W. called most frequently and on which she gave more than a chance number of double calls. It appears, therefore, that there is some relation in the D.W. data between overdoubling a particular call symbol and the peculiarities of displacement that were observed. The actual breakdown of the differences for the three types of displacement in terms of the two groups of call symbols, those that were overdoubled and those that were not, is shown in Table 2. We do not assume that the (+1) and (−1) expectations in each displacement situation are the same for each call sym-

bol, but this suggestion of a relation between displacement and the tendency to overcall a symbol, and particularly to call too many doubles, is one that should be kept in mind for further study as more data from other subjects become available.

Table 2

THE DIFFERENCES BETWEEN (+1) AND (−1) DISPLACEMENT IN RELATION TO THE TENDENCY TO CALL DOUBLES

| Call Symbols | DISPLACEMENT SITUATION | | |
|---|---|---|---|
| | Type I | Type II | Type III |
| Over-doubled Symbols (Circles and Waves)..... | −113 | +54 | + 5 |
| Under-doubled Symbols (Star, Square, Cross)... | − 23 | +29 | −48 |
| TOTAL..... | −136 | +83 | −43 |

The occurrence of a significant degree of avoidance between a call and the target of the following trial *only when that target was missed by the call intended for it* implies that the dissimilarity between the target in question and its own incorrect call was not accidental. If the subject anticipated some of her missed (0) targets and avoided naming these symbols when she was making the preceding (+1) calls, it is hard to think of the calls intended for these same targets as missing them entirely by chance. The facts suggest that more of the card symbols are "singularized" (to use Dr. Thouless' term) as effective objects for ESP response than the high positive score on the assigned targets indicates. Some of the targets are singularized as positive-scoring targets, and these tend to be hit not only by the calls that the subject intended for them but by the preceding calls as well. Other targets are singularized as negative-scoring targets, and these likewise are missed in anticipation by the preceding calls as well as by the calls that the subject intended for them.

The following tentative hypothesis may be offered to explain how the peculiarities of Type II displacement (misses into hits) were produced. On most of her calls, the subject could not identify the assigned target. In her effort to succeed at the ESP task, she was oriented toward the target somewhere ahead of these "blind" areas in the target series. This forward orientation of the subject

led her to anticipate a target which somehow became available as an ESP object before she got to it, and she called the singularized target symbol one step in advance of reaching it and then called it a second time when she was directly at that point in the series of trials. Having responded to the singularized ESP object by naming it correctly twice in succession, however, she avoided calling the same symbol a third time, as she had a strong tendency to avoid making the same call three times in succession. (There are about one-third as many triple calls as would be expected in a random series.) This hypothesis would account for the difference between (+1) and (−1) displacement in the miss-hit-miss sequences of trials if the explanation is to be made in terms of displacement.

Finding a plausible hypothesis for the displacement peculiarities in the Type I situation (pairs of misses) is much more difficult. The D.W. data alone have not thus far suggested any explanation of how the results were produced, and the question will be considered further as more data become available.

Whatever hypothesis should prove to be the correct one, there is more evidence of ESP in the data than methods used heretofore have been able to detect. Even the methods introduced in the present report do not measure the full amount of ESP evidence. They only make it possible to infer that more of this psi process has occurred than has been evaluated. This statement is not intended to imply that all of the subject's responses are determined by his ESP abilities. Very probably most of them are still determined by unidentified psychological factors that have nothing to do with the order of the concealed ESP targets. These will remain in the category of "chance" calls unless further advances in our knowledge of how ESP operates may establish its occurrence in other indirect forms of which we are still ignorant.

## Appendix

### A Method of Evaluating Displacement as an Independent Effect

In this investigation we have taken the hitting on the assigned targets and the subject's habitual sequence of calling as effects that are logically prior to displacement in the D.W. data. That is to say, we have not definitely interpreted any result as due to ESP

displacement so long as it was possible that these other two factors, either singly or in combination, might account for the finding. Therefore, we have not considered that we definitely were dealing with ESP displacement, as such, in any situation except Type I (pairs of misses). In Type II (misses into hits) the "displacement" findings depend wholly upon the occurrence of doubles in the calls in relation to hits on the assigned targets. We have been saying that in Type II displacement the (+1) score is significantly higher than the (−1) score. Another way of stating this same fact is to say that the second of a pair of doubles in the calls hits the assigned target significantly more often than the first of a double. In Type III (hits into misses) the call that makes a displacement hit must also have made a hit on its assigned target. In this instance, hitting the assigned target has been considered the prior effect. In every other type than Type I, then, the findings are equivocal. This does not mean, of course, that the peculiarities discovered may not be attributable at least in part to displacement, nor that it is any less important to study them further.

In Type I, however, there are different ways in which any designated displacement, such as (+1) forward displacement, may be evaluated approximately as an effect independent of both the hitting on the assigned targets and the subject's sequence of calling. One method for doing this is described below as it applies to the (+1) results of D.W. Finally, the outcome of the evaluation of the (−1) score by the same method is also shown.

As the significance of the findings presented in this report arises chiefly from the fact that D.W.'s total forward displacement score was significantly below M.C.E., the evaluation of the (+1) results in a way that takes account of other factors that might influence the displacement score is important. The factors to be taken account of and controlled are: (1) the subject's tendency to call doubles; (2) the rate of scoring on the assigned target; (3) the use of the closed deck; and (4) the effect of (−1) displacement hits.

When a pair of misses has two calls consisting of the same symbol, there can be no displacement hit in either direction. In Type I cases, therefore, a tendency to call an excessive number of doubles would have the effect of producing a relatively low displacement score, and *vice versa*. In order to rule out the influence of D.W.'s

doubles in the calls, pairs of misses with doubles in the calls may be omitted in evaluating the evidence for Type I forward displacement.

The effects of the hitting on the assigned targets are controlled, within reasonable limits, by the fact that no (0) hits are directly involved in Type I forward displacement. The only qualification needed at this point would be on the question of whether the missed targets are sufficiently close to forming a random series.

It is necessary to consider separately those Type I cases in which there was no backward displacement hit, as in $\begin{matrix} A & B \\ C & ? \end{matrix}$, and those Type I pairs in which backward displacement hits did occur, as in $\begin{matrix} A & B \\ B & ? \end{matrix}$. At the same time, the fact that a closed deck was used must be kept in mind. The question to be examined is that of how the probability of occurrence of a designated target symbol in the second card position, that marked by the question mark, is affected by these factors.

In the absence of a (−1) hit, there are 19 cards that can appear in the second card position as the forward displacement target, and five of these will give a (+1) hit. In the situation $\begin{matrix} A & B \\ C & ? \end{matrix}$, for instance, there are six cards among the 25 cards in the deck that cannot appear at the point indicated by the question mark. The five C cards are excluded by the fact that this trial must be a miss; and the single B card which is used up in making the first trial a miss is also excluded. All of the A cards are still eligible as possible forward displacement targets. On the assumption that the occurrence of any one of the 19 possible cards at the point in question is a random event, the probability of a (+1) hit is 5/19.

In the D.W. series, there are 1,928 Type I situations (pairs of misses) in which no backward displacement hits occur. Therefore the expected (+1) score for these cases is (1928 × 5/19), or 507.37. As an approximation, the variance may be taken as $npq$, where $n$ is the number of cases, $p$ is the probability of a displacement hit, and $q$ is $(1-p)$. This will not give the exact variance because of the use of the closed deck, but previous studies show that the correct variance for the closed deck situation is only slightly different from that for the binomial distribution. Approximately, therefore, the variance is (1928 × 5/19 × 14/19), or 373.85.

In Type I cases (pairs of misses) in which (−1) hits are made, the use of the closed deck leads to a different probability of scoring a (+1) hit. In the situation $\begin{matrix} A & B \\ B & ? \end{matrix}$ it is possible for the second card, the one indicated by the question mark as the forward displacement target, to be any symbol except B, the one appearing in the first card position. The forward displacement target is equally likely, therefore, to be A, C, D, or E, and the probability that the first call will hit the second card is 1/4.

In the D.W. series, there are 691 Type I cases in which (−1) hits occur, so the expected (+1) score is 691/4, or 172.75. The approximate variance in this instance is (691 × 1/4 × 3/4), or 129.56.

The total Type I (+1) score for D.W. was 555 hits. By the above method, the total expectation is 507.37 + 172.75, or 680.12. The observed (+1) score thus represents a deviation from "chance" of −125.12. The SD is the square root of the total variance, $\sqrt{373.85 + 129.56}$ or 22.44. This gives a CR of −5.58 ($P < 10^{-8}$) for the forward displacement score in pairs of misses.

The evaluation by the above method of the (−1) score in the pairs of misses gave the following results: (−1) deviation, +8.57 hits; SD, 22.47; CR, +.38. Thus it is clear that the observed backward displacement score is quite close to M.C.E., and it follows that the significant difference between (+1) and (−1) in Type I displacement is almost entirely due to the negative deviation on forward displacement.

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# A STUDY OF CERTAIN PSYCHOLOGICAL FACTORS IN RELATION TO ESP PERFORMANCE[1]

By Leo Eilbert and Gertrude R. Schmeidler

**ABSTRACT: This experiment was designed to study the relationship between the ESP scores of 100 subjects and such factors as work habits, frustration, attitude toward the experiment, and listening to music. In a total of 500 runs of ESP cards done by these subjects the results were not significantly different from mean chance expectation. Each subject was asked, after completing his five runs and before the results were checked, on which run he had done the best. The 69 subjects who made such choices were successful as a group to a statistically significant degree in picking their best runs.**

**When the subjects were rated with regard to work habits, the group classed as "ego involved" had a total ESP score which was below mean chance expectation, and the group classed as "task oriented" had a total score which was above the expected average. The difference between the scores of these two groups was significant.**

**When 49 of the subjects were rated on the basis of their responses on the Rosenzweig Picture-Frustration Study, it was found that as a group those who tended to turn their aggression outward made a lower total ESP score than did the subjects whose aggression was expressed less overtly.**

**The results of the experiment are in line with previous findings inasmuch as the group of subjects who believed ESP to be possible had a total score above mean chance expectation and those who were doubtful or who completely rejected the possibility of ESP scored below, the difference being highly suggestive statistically.**

**No significant difference was found between runs done with music and runs done without music.**

**The text also discusses differences in results between subjects tested at the experimenter's home and subjects tested at the College and compares the results obtained from testing friends, acquaintances, and strangers.**

**Mr. Eilbert is doing graduate work in psychology for his Ph.D. degree at the University of Pittsburgh. He also teaches psychology at Duquesne University. Dr. Schmeidler is already known to Journal readers.—Ed.**

THE THESIS which this experiment was designed to test is that both the relatively stable attitudes of a subject and his evanescent moods are important in determining his success in ESP. Since music often influences mood, the initial procedure compared the

[1] The experiment constituted the material for a thesis submitted by L.E. to the School of Education, the City College of New York, in partial fulfillment of the requirements for the degree of Master of Science in Education. All experimenting was done by L.E. G.R.S., who acted as chairman of the thesis committee, shared in planning the research and in the analysis of the data.

ESP scores made by fifty subjects while they were listening to different kinds of music with their ESP scores when no music was played. Observations of the subjects' behavior were recorded, other questions prompted by their comments were explored, and certain leads were uncovered which seemed promising. The emphasis of the experiment shifted accordingly; and in the second half, fifty new subjects were tested under conditions which were formally similar to the earlier ones but which included a more detailed study of their work habits, their attitude toward the experiment, and their reactions to frustration.

## Procedure

The basic design of the experiment was simple. Each subject was required to make five ESP runs and was asked to guess which was his best run. All ESP data relate either to ESP score or to success in choice of the best run.

Many additions were made to this basic design. In the interest of clarity, they will be described under separate headings. The final heading, "Administration of the Experiment," presents in summary form a composite picture of the entire procedure.

### *ESP Materials*

Before each half of the experiment, an associate prepared five decks of ESP cards from a list of random numbers. The decks were used as targets by fifty subjects, each subject guessing at each deck. Since there is theoretically a possibility that subjects might tend to call the symbols in similar patterns in the various runs, the decks were rotated systematically. Each deck was used an equal number of times as the first, the second, the third, the fourth, and the fifth in the session. Each deck was also associated an equal number of times with each of the five periods of music or silence that are described in the following section. We may anticipate the results by adding that at the end of the experiment, when ESP scores were compared, no list showed an appreciably higher score than any of the others. The usual statistical procedures were therefore employed, since it seemed safe to assume that any "stacking effect" which may have occurred in these series was not large enough to produce spurious results.

The associate also made a permanent record of the decks and gave these lists to G.R.S. They were shown to L.E. only after all subjects who were to guess at them had been tested.

The first fifty subjects were tested by a modified, screened BT method; the second fifty subjects used the DT method with sealed decks. All guesses were recorded by the subjects on record blanks. All subjects were asked, "On which part do you think you did best?" If they replied, the answer was recorded by the experimenter, who next inquired why they thought so, and recorded this response also. All ESP runs were independently scored by L.E. and by an associate. Some further details will be described under "Administration of the Experiment."

*Music*

Every subject made one ESP run without music, and one while each of four musical recordings was played. Thus five ESP runs were made while listening to:

(1) No music.

(2) A popular classical record which it was hoped would have a pleasant and relaxing effect ("Clair de Lune," No. 3 from "Suite Bergamesque," Debussy; Jose Iturbi, pianist. RCA Victor, 11-8851).

(3) A loud, rather violent jazz record ("Bugle Call Rag," Benny Goodman and his orchestra; The Dixieland Band. Columbia, 36109).

(4) A quieter jazz record ("Blue Skies," Irving Berlin; Perry Como, vocalist, and the "Satisfiers" with Russ Case and his orchestra. RCA Victor, 20-1917).

(5) An achromatic Japanese vocal record (Hauta Umenino Haru. Decca, 23174).

The records were played on a portable phonograph. The same records were used throughout the experiment.

In order to compare the subject's responses to the various records and to the period when no music was played, it was necessary to control any possible primacy and finality effects, or intraserial contrast and reinforcement. The order of the five intervals was therefore varied systematically. Since 120 permutations are possible, a 5 x 24 table was made. The first column listed the 24 permutations in

which no music was played during the first run; the second column listed the 24 permutations in which "Clair de Lune" was played during the first run, and so on. The table was numbered along the rows, so that successive numbers in each column were five units apart, as: 1, 6, 11 . . . 116. Subjects were assigned numbers which corresponded to the order in which they took part in the experiment. Records were played for each subject in the sequence which corresponded to his number in the table.

*Work Habits*

When all other parts of the experiment had been completed, each subject was asked, "Under what conditions do you generally do your best work?" Subjects' answers to this question were listed under one of three headings: (1) egocentric, those which referred to the mood, comfort, or interests of the subject; (2) goal driven, those which stated that the subject worked best under pressure; (3) task oriented, those which referred to the environmental conditions that were most conducive to work. These listings were made by a judge who did not know the subjects' ESP scores. Thirty subjects were also asked to place themselves in one of the three following categories: (1) I work best under optimum environmental conditions; (2) I work best under pressure; (3) I work best when relaxed or when interested in the material. Comparisons were made between the judge's placement of the subject and the subject's placement of himself.

*Reactions to Frustration*

The Rosenzweig Picture-Frustration Study was administered to the second fifty subjects. This projective test has been discussed in detail elsewhere (1, 3) and need therefore be described only briefly here. Its purpose is to determine how a subject typically responds to moderate frustration. It consists of twenty-four line drawings, each portraying a scene in which two or more persons take part, one of whom is undergoing what most of us would consider a frustrating experience. A balloon sketched above another character, as in a comic strip, contains the words that he or she is saying, while an empty balloon is sketched above the frustrated individual. It is the subject's task to write in it, for each of the twenty-four cartoons, the first reply that comes to his mind.

The twenty-four responses may be analyzed according to several different categories, of which only three will be described. They refer to the direction of the subject's aggression, and are defined by Rosenzweig as follows (1):

"*Extrapunitiveness:* Aggression is employed overtly and directed toward the personal or impersonal environment in the form of emphasizing the extent of the frustrating situation, blaming an outside agency for the frustration, or placing some other person under obligation to solve the problem in hand.

"*Intropunitiveness:* Aggression is employed overtly, but directed by the subject against himself in the form of a martyr-like acceptance of the frustration as beneficial, acknowledgment of guilt or shame, or an assumption of responsibility for correcting the frustrating situation.

"*Impunitiveness:* Aggression is evaded or avoided in any overt form, and the frustrating situation is described as insignificant, as no one's fault, or as likely to be ameliorated by just waiting or conforming."

Administration of the test consisted in this case of giving the test blanks to the subject, but not of inquiring about his responses. One of the records omitted four responses, and is therefore discarded.

All responses were independently scored by the two authors. Discrepancies were checked by G.R.S., who made all final decisions as to the category which was more consistent with the published scoring samples (3). These decisions were made before G.R.S. knew the subjects' ESP scores.

*Attitude Toward the Experiment*

Information about the subject's attitude toward the experiment has been derived from several sources. One was L.E.'s notes on spontaneous comments and behavior, varying from such favorable responses as an offer to act again as subject, to such unfavorable ones as, "When this (the last run) is done, will it (the experiment) be over?"

A second source of information, for the latter half of the experiment, came from conversation about ESP. During a brief preliminary discussion with the subject, L.E. attempted to determine his

views. The subject's comments were recorded, and L.E. assigned each (without knowledge of his ESP score) to one of the following five categories: (1) believes in ESP and thinks he will do well in this experiment; (2) believes in ESP but doubts that he will do well in this experiment; (3) doubtful about the whole subject; (4) rejects ESP completely; (5) gives irrelevant or contradictory responses.

A third attempt to measure the subject's attitude consisted of administering after each ESP run a miniature questionnaire, which was mimeographed at the bottom of each ESP record blank. It inquired as to whether the subject had felt relaxed, stimulated, or annoyed during the test, or had felt no effect from the music.

A fourth source of information which may relate to attitude comes from a listing of two background conditions: the subject's relation to the experimenter, and the place where the experiment was performed. All subjects were listed as friends, acquaintances, or strangers. A record was also made of whether the experiment was performed at City College or in L.E.'s home, which was an inconvenient location for most of the subjects. (No attempt was made to estimate the degree of inconvenience for each individual.)

*Administration of the Experiment*

Subjects 1-28 made an appointment to come to L.E.'s home for the experiment. Subjects sometimes came earlier or later than they had agreed. The subject who was too early, or the subject who followed one who was too late, often interrupted an experiment, and in that case was hurriedly ushered into another room where he waited until the experimenter was ready for him.

Subjects 29-100 were tested at the College. They did not make advance appointments. Instead, L.E. would try to find a subject when he was ready for one. Delays for the subject were minimal, as were interruptions once the experiment was in progress.

For the first fifty subjects, the experiment was scheduled to take one-half an hour. Unless the subject hurried, it often ran overtime. The beginning of the experimental session was devoted to the experimenter's describing the general nature of ESP and the procedure which was to be followed. The subject was told that many other people had guessed at the cards in much the same manner that he was to do and that the results had been found to exceed

chance expectancy. The five different ESP cards were shown him. On each card was written the symbol by which the subject was to identify it in recording his guesses. These five cards were left on the table during the experiment to ensure that it was ESP ability and not differential forgetting of the symbols that was being tested. The subject was presented with five record blanks numbered in the order in which they were to be used. Each blank had twenty-five spaces for ESP guesses and the questions at the bottom as to the subject's mood during the ESP run. The subject was told that he was to guess the top card of the deck, which the experimenter would hold face down, screened so that it was out of sight. When the subject had recorded his guess, the card would be removed; he was then to guess at the next top card, and so on through the deck.

Before four of the runs, the subject was informed that a phonograph record was to be played. The experimenter told him the name of the record and prepared him for the type of music he was to hear. (This portion of the procedure was substantially the same for all the subjects, except that the records grew more scratchy during the course of the experiment.)

For subjects 51-100, no time limit was set for the session. The experimental period began with a discussion, rather than an explanation, of the nature of ESP. The experimenter endeavored during this period to determine the subject's attitude toward the ESP guesses, rather than to convey his own. As before, the sample ESP cards were shown and were left on the table during the session. The five target decks, each sealed in an opaque cardboard container, and five numbered record blanks were also on the table. The subject was told the procedure, was advised to guess at the cards in order, and was asked to handle the deck at which he was guessing. In the hope that taking an active part in the experiment might make for more feeling of participation, he was also asked to use the decks and the corresponding test blanks in the order in which these materials were arranged. It was he who put the used decks and completed blanks aside and reached out to get the next ones. When the five ESP runs had been completed, the Rosenzweig Picture-Frustration test was administered.

For all subjects, the experimenter made notes during and immediately after the session as to behavior and spontaneous com-

ments. At the conclusion of the ESP runs for subjects 1-50, and at the conclusion of the Picture-Frustration test for subjects 51-100, the experimenter asked on which of the ESP runs the subject thought he had done best. Some subjects were unwilling to answer, and they were not required to do so. Those who responded were next asked why they had made that choice. The experimenter then inquired about work habits. Not until fifty subjects had completed the experiment did the experimenter see the target lists or score any of the ESP records.

## Results

In this section the data will be summarized under separate headings, corresponding to the preceding ones. Statistical evaluations will be presented, but attempts to determine their meaning will be postponed until the next section.

### *ESP Totals*

The overall results of the ESP guesses in the two halves of the experiment are listed in Table 1. As in so much of the research in ESP performed at City College, the grand total is very close to mean chance expectation, and meaningful relations appear only when the data obtained under different conditions are compared with each other.

Table 1

Scores on ESP Runs

| Series | Number of Subjects | Runs | Dev. |
|---|---|---|---|
| Series 1 | 50 | 250 | −40 |
| Series 2 | 50 | 250 | +35 |
| Total | 100 | 500 | − 5 |

It will be remembered that all subjects were asked on which run they thought they had done best, but that not all subjects gave responsive answers. Evaluation of the 69 guesses that were made is complicated by the fact that some subjects' scores showed a two-way or three-way tie for the most successful run. For them, the probability of guessing the "best run" correctly rises from one-fifth to two-fifths or three-fifths. We have used the following formula to evaluate the critical ratio for these data:

$$CR = \frac{Dev_1 + Dev_2 + Dev_3}{\sqrt{N_1(p_1q_1) + N_2(p_2q_2) + N_3(p_3q_3)}}$$

The subscript *1* refers to the cases where one run received a higher score than the others, so that $p$ is $\frac{1}{5}$ and $q$ is $\frac{4}{5}$. The subscript *2* refers to the cases where two of the five runs were tied for highest score, so that $p$ is $\frac{2}{5}$ and $q$ is $\frac{3}{5}$. The subscript *3* refers to the cases where three of the five runs were tied for highest score, so that $p$ is $\frac{3}{5}$ and $q$ is $\frac{2}{5}$.

Table 2 shows more correct guesses than would be expected by "chance," (P = .002).[2] This indicates that the subjects who were willing to respond to the question, "On which part do you think you did best?" showed some ability to estimate correctly when they were most successful at ESP.

Table 2

"Second Order ESP" Guesses
Subject's Choice When Asked to Select His Best ESP Run

| | Number of Guesses | Number of Correct Guesses | Dev. | CR |
|---|---|---|---|---|
| When one run was best..... | 45 | 16 | + 7 | 2.61 |
| When two runs tied for best... | 19 | 9 | + 1.4 | .65 |
| When three runs tied for best.. | 5 | 5 | + 2 | 1.82 |
| Total..... | 69 | 30 | +10.4 | 2.89 |

*Music*

ESP scores are analyzed in Table 3 according to the music that was being played while the guesses were made. Scores averaged slightly below "chance" when there was no music and slightly above "chance" when there was music. The difference is not significant.

[2] P, as used in this paper, represents the area under the normal probability curve in only one tail of the distribution. It may be regarded as a test of the hypothesis that the scores of the selected runs represent a sample from a population of run scores in which the mean is less than or equal to that which would be expected on a chance basis. If the test leads to rejection of this hypothesis, it logically implies acceptance of the contrary hypothesis; namely, that the mean of the selected run population is greater than the chance mean. When a certain prediction has been made before the experiment as to the direction the data will take and the data do in fact take that direction, P, as thus defined, seems an appropriate measure of the significance of the findings. When no prediction had been made before the data were examined, 2P is a more appropriate measure of significance and has been so used in this article. It is hardly necessary to add that, when the results are different from what had been predicted, the most appropriate test of significance is experimental rather than statistical, and consists of repeating the experiment or re-examining earlier data to see whether the observed trend appears consistently.

Among the musical selections, the two which were quieter ("Clair de Lune" and "Blue Skies") were associated with lower ESP scores than the two which were noisier ("Bugle Call Rag" and the Japanese music). This difference, also, is not significant. Scores made while listening to the two noisier records averaged 5.2. This is substantially higher than the mean score when no music was played (4.7); and the difference between them is suggestive, but not significant (2P = .05).

Table 3

ESP Scores Made in Four Periods with Music and in One Period without Music

| Music | Number of Subjects | Runs | Dev. |
|---|---|---|---|
| No music | 100 | 100 | −27 |
| "Clair de Lune" | 100 | 100 | − 9 |
| "Blue Skies" | 100 | 100 | −10 |
| "Bugle Call Rag" | 100 | 100 | +19 |
| Japanese Music | 100 | 100 | +22 |

*Work Habits*

The work habits reported by the subjects were listed under three categories: egocentric, goal driven, and task oriented. The ESP scores of the egocentric and goal driven subjects were below "chance," and those of the task oriented subjects were above "chance." The difference between goal driven and task oriented subjects is statistically significant at the level of 2P = .002.

Table 4

ESP Scores of Subjects Classified According to Their Answer to the Question, "Under What Conditions Do You Generally Do Your Best Work?"

| Classification | Runs | Dev. | *t* value of difference from "task oriented" | 2P |
|---|---|---|---|---|
| Egocentric | 180 | −25 | 1.82 | .07 |
| Goal driven | 55 | −37 | 2.99 | .002 |
| Task oriented | 265 | +57 | .... | .... |
| Ego involved* | 235 | −62 | 2.68 | .008 |

*"Ego involved" represents the subjects who are listed as either "egocentric" or "goal driven."

The data on work habits were also classified in another way: the two categories of "egocentric" and "goal driven" were combined

under the single heading of "ego involved," the term which is customarily contrasted with "task oriented." When this conventional dichotomy is used, the difference between the ESP scores of the two groups is significant at the level of $2P = .008$.

Thirty subjects were asked to classify themselves according to the three original categories. Twelve of these self-ratings were consistent with the judge's ratings. Eleven were inconsistent, and seven subjects were unwilling to place themselves in only one of the three groups. The ESP scores of the twelve subjects whose self-ratings were consistent with ratings later made by the judge had a higher average ESP score than did the other eighteen subjects.

*Reaction to Frustration*

Norms for the Picture-Frustration Study have been published by Rosenzweig (1) who presents quartiles for fifty males employed in a large manufacturing plant, for fifty male medical interns, and for fifty normal females. In a follow-up study which is as yet unpublished, he reports that groups subsequently tested have given scores very similar to these (2). Also available for comparison are unpublished data on 446 subjects tested by G.R.S. Since most of these subjects, like those of the present experiment, were undergraduates at City College at the time of taking the test, comparison with G.R.S.'s data is probably somewhat more appropriate than with Rosenzweig's. Tables 5 and 6 compare the ESP scores of subjects scoring in the highest quartiles of extrapunitiveness, intropunitiveness, and impunitiveness with the ESP scores of subjects scoring in the lowest quartiles of these traits. In Table 5 the quartiles are determined by comparison with Rosenzweig's norms; in Table 6 by comparison with G.R.S.'s.

Table 5

ESP Scores in Relation to Extrapunitiveness, Intropunitiveness, and Impunitiveness (Using Rosenzweig's Published Quartiles)

| | First Quartile | | Fourth Quartile | | *t* value of difference | P* |
|---|---|---|---|---|---|---|
| | Runs | Dev. | Runs | Dev. | | |
| Extrapunitiveness | 25 | +15 | 135 | − 7 | 1.49 | .07 |
| Intropunitiveness | 90 | 0 | 15 | +18 | 2.14 | .02 |
| Impunitiveness | 90 | − 3 | 45 | +24 | 1.55 | .06 |

* P, rather than 2P, is used because the trend of the data is consistent with the results of other research (5).

Table 6

ESP Scores in Relation to Extrapunitiveness, Intropunitiveness, and Impunitiveness (Using G.R.S.'s Quartiles)

| | First Quartile | | Fourth Quartile | | *t* value of difference | P* |
|---|---|---|---|---|---|---|
| | Runs | Dev. | Runs | Dev. | | |
| Extrapunitiveness | 65 | +21 | 45 | −12 | 1.69 | .05 |
| Intropunitiveness | 65 | − 2 | 50 | +32 | 1.79 | .04 |
| Impunitiveness | 45 | −14 | 75 | +20 | 1.52 | .07 |

* P, rather than 2P, is used because the trend of the data is consistent with the results of other research (5).

These tables show that extrapunitive subjects tended to make lower ESP scores than those who were less extrapunitive, and that impunitive or intropunitive subjects tended to make higher ESP scores than those who were less impunitive or intropunitive. The differences are not statistically significant.

Table 7 lists the correlations between ESP scores and extrapunitiveness, intropunitiveness, and impunitiveness. The correlation for extrapunitiveness is significant at the level of P = .01. The other correlations are not significant.

Table 7

Correlations between ESP Score and Extrapunitiveness, Intropunitiveness, and Impunitiveness

| | *r* | *t* | P* |
|---|---|---|---|
| Extrapunitiveness | −.32 | 2.28 | .01 |
| Intropunitiveness | +.28 | 1.71 | .04 |
| Impunitiveness | +.22 | 1.51 | .07 |

* P, rather than 2P, is used because the trend of the data is consistent with the results of other research (5).

*Attitude Toward the Experiment*

It will be remembered that in the second series the subjects' acceptance or rejection of the possibility of paranormal success was evaluated by the experimenter. Table 8 shows that the 15 runs made by subjects classed as "believes in ESP and thinks he will do well in this experiment" had a mean score of 5.5. Subjects classed as "believes in ESP but doubts that he will do well in this experiment" made 55 runs, which had a mean score of 5.6. All other categories had mean scores very slightly below chance. None of

these scores is significant. If the scores of the subjects who were classed as believing in ESP (Table 8, items 1 and 2) are compared with the scores of the subjects who were classed as being doubtful about the whole subject or rejecting ESP completely (Table 8, items 3 and 4), the difference between them is significant at the level of P = .02.

Table 8

RELATION BETWEEN ESP SCORE AND THE ACCEPTANCE OF THE POSSIBILITY OF PARANORMAL SUCCESS

| Classification of Subject | Runs | Dev. |
|---|---|---|
| 1. Believes in ESP and thinks he will do well in this experiment | 15 | + 8 |
| 2. Believes in ESP but doubts he will do well in this experiment | 55 | +33 |
| 3. Doubtful about the whole subject | 115 | − 2 |
| 4. Rejects ESP completely | 20 | − 2 |
| 5. Gives irrelevant or contradictory responses | 45 | − 2 |

Subjects had been asked to check after each ESP run whether they found that the music had a relaxing, stimulating, or annoying effect, or no effect. Those who checked "relaxing" or "stimulating" had average ESP scores slightly above chance; those who checked "annoying" or "no effect" had average ESP scores slightly below chance. The differences were not significant.

Table 9 shows the scores of subjects tested at L.E.'s home, who scored somewhat below chance. The mean score of their 140 runs is 4.71, which is not significantly different from chance. They were all tested in Series 1. When their scores are compared with those of the other subjects tested in Series 1 the difference is not significant; but when their scores are compared with those of all subjects tested at the college, both in Series 1 and Series 2, the difference is suggestive (2P = .05). It is questionable whether this latter comparison is legitimate, since several other factors were varied in Series 2, as has been reported under "Procedure."

When the first half of the experiment was completed, subjects were divided into those who were friends of L.E., those who were acquaintances, and those who, before the experiment, had been strangers to him. This analysis (Table 10) showed that acquaintances had lower ESP scores than either strangers or friends. The differences were suggestive. A similar analysis for the second half

Table 9

ESP Scores of Subjects Tested at L.E.'s Home and of Subjects Tested at the College

| Where Tested | Series 1 | | | | Series 2 | | | | Total | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Runs | Dev. | $t_d$ | 2P | Runs | Dev. | $t_d$ | 2P | Runs | Dev. | $t_d$ | 2P |
| At L.E.'s home... | 140 | −41 | 1.18 | .24 | 0 | | | | 140 | −41 | 1.97 | .05 |
| At college..... | 110 | + 1 | | | 250 | +35 | .... | .... | 360 | +36 | | |

of the experiment showed again that the scores of acquaintances were somewhat lower than those of strangers, but for this series the differences were slight. When all subjects were compared, the difference between ESP scores of acquaintances and strangers is suggestive (2P = .02) and the difference between ESP scores of acquaintances and of all other subjects (both friends and strangers) is significant at the level of 2P = .01.

Table 10

ESP Scores of Subjects Who Were the Experimenter's Friends or Acquaintances and of Subjects Who Were Strangers to Him

| Classification | Series 1 | | | | Series 2 | | | | Total | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Runs | Dev. | $t_d$ | 2P | Runs | Dev. | $t_d$ | 2P | Runs | Dev. | $t_d$ | 2P |
| Friends..... | 50 | + 8 | | | 0 | .... | | | 50 | + 8 | | |
| Strangers..... | 65 | +13 | | | 205 | +31 | | | 270 | +44 | | |
| Total..... | 115 | +21 | 2.41 | .02 | 205 | +31 | .19 | .. | 320 | +52 | 2.56 | .01 |
| Acquaintances.... | 135 | −61 | | | 45 | + 4 | | | 180 | −57 | | |

The experimenter's notes on the behavior and comments of the subjects do not lend themselves readily to quantitative treatment or tabular presentation. We shall not attempt to summarize them all, but shall present an account of two sample cases.

One of the first subjects to be tested at the College was a girl who was trying to register for a course on the last day that registration was permitted. As she was not a City College student she was not familiar with the procedure. L.E. offered to help her if she would take part in the experiment. She accepted hesitantly,

displaying a good deal of reluctance. As there was only one hour between the time that the experiment was begun and the close of registration, the test was hurried. Her ESP scores were unusually low (Av. Run Score = 3.2). The subject's comment, without knowledge of her score, was, "This was *not* the ideal situation!"

Another subject, a young man under Reichian analysis, had a total score exactly equal to chance expectancy. Individual runs showed considerable variability: the scores were 7, 0, 1, 10, 7. The two low scores were obtained while the subject had been listening to "Clair de Lune" and "Blue Skies." While these two records were being played he had worked very rapidly and had made a spontaneous comment about wanting to get done (with the ESP runs) so that he could listen to the music. The combination of impatience and low ESP scores suggested data obtained by Soal and Goldney in working with B.S., the famous subject who was so impatient in temperament, so unsuccessful in hitting the target at which he was aiming—and so extraordinarily successful in hitting the targets one or two places ahead of the proper ones (6). To find whether a similar pattern was occurring here, the two runs with scores of 0 and 1 were matched with the symbol after the target—that is, scored for (+1) forward displacement. This gave scores close to chance expectancy. The same runs were next matched with the second symbol after the target—that is, scored for (+2) forward displacement. These scores were 9 and 8. This is, of course, consistent with the performance of B.S.

## Discussion

### *ESP Totals*

In considering the total ESP scores, we observe that the overall deviation of the 500 ESP runs is very close to chance expectancy, but that the subjects' estimates as to which of their ESP runs was the best were somewhat better than chance. At first glance it seems curious that the two methods of making ESP guesses should have yielded disparate results; but if we examine the conditions under which the guesses were made we find several differences which may, singly or jointly, account for the higher scores in choosing the best run: (1) The experimenter was using a novel approach. Many workers feel that liveliness of interest on the part of the experi-

menter, encouraged by his feeling of being on a new path, modifies the interpersonal relations between him and his subject so as to increase the likelihood of the subject's success. (2) The subject wrote his standard ESP guesses on a prepared blank, using the prescribed symbols. When he guessed his best run the conditions were less formal in three respects: the question was put to him conversationally by the experimenter; he answered orally; he was allowed to use his own words to describe his choice. (3) All subjects completed all ESP runs, but some subjects were permitted to abstain from guessing on which run they did best. Forced guesses, on which the subject's willingness and spontaneity were minimal, were thus required in the first case but not the second. (4) The subject was guessing about his own responses. This may have made him feel more intimately involved with the task. Obtaining the right level of involvement offers a difficult psychological problem, for we can be almost certain that the optimum for ESP success is neither the minimum nor (in the laboratory at least) the maximum. But perhaps this elusive optimum was approximated here. (5) There may be a certain advantage in having ESP guesses of ESP guesses, a "second order ESP." If so, we could expect to have scores still higher if subjects should be asked after twenty-five such runs to make a "third order ESP" guess, and choose the best among their previous five slections. This hypothesis implies a progressive sifting and refinement of ESP choices, whose eventual limit would be determined only by the subject's attention span.

*Music*

The lack of a significant relationship between music played while making ESP guesses and success in those guesses may point up a problem common to much research in psychology as well as in parapsychology. Too many of us, finding at the end of an experiment that our data are ambiguous or suggestive instead of clearcut and significant, have been wise after the event and have realized that our difficulty came from having defined the problem in terms of the physical stimulus instead of the psychobiological environment. The sounds of the music were similar for all subjects, but one thought the Japanese music stimulating and another thought it weird and unpleasant. One relaxed and worked without tension when he

heard the pleasant tones of "Clair de Lune," but another felt compelled to hurry tensely through his ESP guesses while "Clair de Lune" was being played so that, with the guesses out of the way, he could sit back and enjoy the music. It is necessary, therefore, to discount the predictive value of the suggestive differences of this part of the experiment. We should probably base all our conclusions on analyses couched in terms of the subject's reactions rather than in terms of the objective stimulating situation.

*Work Habits*

There is an obvious danger in classifying a subject in respect to such a general trait as being task oriented or ego involved on the basis of his answer to a single question. Perhaps the reason that the classification seems so successful in this instance is that the question tapped the subject's responses to a situation essentially similar to the experimental one. A stickler for precision, instead of heading the first classification in Table 4 "Egocentric," might have called it "Egocentric in respect to reporting the conditions under which he generally does his best work, and therefore perhaps also egocentric in his responses in this experiment." Granted, then, that the trait name should be recognized as applying in a specific rather than a global sense, it is not unexpected that the observed differences should have appeared. One of us (G.R.S.) remembers a conference at Duke University's Parapsychology Laboratory some years ago which she attended with paper and pencil to take notes on the characteristics of the subject who would be expected to score well at ESP or PK. The discussion soon turned away from the initial topic, and at the end she had written only one notation, which we quote in full: "Task oriented probably better than ego involved." The similarity between this prediction and the data of Table 4 needs no comment.

If we look for some psychological meaning behind this relationship, we may find it in the greater interest that the task oriented subject will be likely to show in an experiment that has little personal meaning for him. Probably the ego involved person will consciously or unconsciously reject his role as a subject in a predetermined procedure which will be modified very little even for HIM, and where HIS results are sure to lose their identity when

they are averaged with everyone else's. If this is the explanation for what we have found, it reconciles the apparent paradox that, while ego involved subjects gave poorer laboratory scores than task involved subjects, some of the most spectacularly successful psychics seem to have been extraordinarily egocentric. Where the ego involved person can help to choose the conditions of each session and can command the awed attention of his audience, the psychological setting is of course very different from that of the routinized research which we describe. It should be possible to test this theory experimentally by determining whether the advantages of the highly individualized over the group experiment are greater for the subjects who are more ego involved.

*Reactions to Frustration*

Scores on Rosenzweig's Picture-Frustration Study showed a relationship to ESP score, in that the more extrapunitive subjects tended to have lower ESP scores and the less extrapunitive subjects tended to have higher ones. This trend has been confirmed by evidence from 446 additional subjects (5). Let us consider what it might mean.

The extrapunitive scores on this test denote a free or relatively free display of aggression when subjects are faced with moderately frustrating situations. The more extrapunitive subjects are, in general, the ones who have least tolerance for frustration and whose behavior shows the most marked lack of rapport when frustration occurs. The less extrapunitive subjects might be more likely to accept the responsibility for active cooperation or to minimize the frustration, to smooth it over pleasantly, and act as if nothing was wrong.

The ESP experiment, like the situations pictured in the frustration test, was interpreted as moderately annoying by some of the subjects. Presumably the more extrapunitive subjects were more likely to behave aggressively as a result of such annoyance, while the others were more likely to behave pleasantly and cooperatively. Since most research in ESP that deals with this question has indicated that pleasantly cooperative subjects will make higher scores than antagonistic, aggressive ones, the obtained trend becomes understandable.

Since the trend is so slight, and only one of the calculations of Tables 5, 6, and 7 is significant at the level of P = .01, it is worth examining the individual records to find hints as to what other factors may be operative. Two emerge. One deals with the question of whether or not a subject will behave characteristically in the experimental situation. Subjects who were typically extrapunitive but who enjoyed the experiment might, for example, be expected to act cooperatively during the session. A typically impunitive subject who was pressed for time, caught between reluctance to participate and reluctance to refuse, might be experiencing so much frustration that her responses would be more extrapunitive than her Rosenzweig Picture-Frustration scores would have indicated. Thus the amount of frustration experienced by the subject, as well as his typical patterns of response, should be taken into account. Unfortunately we have no quantitative measure of the former.

The second question deals with the direction of the subject's aggression. Two of L.E.'s closest friends, both of whom were busy men, were extremely extrapunitive *to him* before, during, and after the experimental session, presumably exaggerating whatever frustration they felt. Did this freedom of expression cheer them up? Their ESP scores for the five runs were 30 and 33. Perhaps the opportunities for emotional release which a situation affords should also be taken into account; the extrapunitive subject who feels he must be polite to the experimenter may be so much the more aggressive to the experiment.

*Attitude Toward the Experiment*

Previous research dealing with the subject's attitude toward the likelihood of paranormal success has shown that subjects who accept the possibility of ESP success in their guesses made somewhat higher average scores than those who rejected all possibility of it (4). In the current experiment a four-part division was used instead of a dichotomy; and in these data the breaking point occurred between the two middle categories. The subjects who were classed as believing in ESP and thinking they would do well in this experiment and those who were classed as believing in ESP but doubting that they would do well in this experiment both had average ESP scores above mean chance expectation. The subjects who were

classed as doubtful about the whole topic and those who were classed as rejecting ESP completely both had average ESP scores very close to, but slightly below, mean chance expectation. The data are too few (or the differences are too small) to be significant, but they indicate that the cruder dichotomous division used in most of G.R.S.'s earlier experiments obscured some points which a finer analysis would have demonstrated.

Subjects' verbal reports of their own mood are notoriously difficult to used for productive research. Probably one reason is that subjects who are not trained introspectionists have differing connotations, and thus differing usage, for the same terms; and perhaps another reason is that the mechanism of repression operates to keep many individuals from recognizing what their own reactions were. In the present experiment a suggestively but not significantly higher average ESP score was shown on runs where subjects reported they had felt relaxed or stimulated, as opposed to the runs where they reported they had felt annoyed or had observed "no effect." This is consistent with observations made in other research, where optimal conditions for ESP success have been described in terms of lively interest or of ease and relaxation, or sometimes of a combination of the two such as is implied in Carington's phrase, "an attitude of almost nonchalant receptivity."

An unanticipated result of the experiment was the contrast between the low average ESP scores of L.E.'s acquaintances and the higher average ESP scores of strangers and friends. Perhaps the explanation lies in the fact that acquaintances knew L.E. well enough to feel obliged to help him when he asked, unless there was an urgent reason for refusing. Strangers would feel little compunction in refusing his request; therefore most of the strangers who complied with it would be those who felt they had time to act as subjects and expected to enjoy doing so. Friends, of course, would ordinarily be glad to help L.E. and would be able, since they saw him so often, to decide with him on the time that was most convenient for both. Thus it may be that of these three classes, the acquaintances would be the only group where a considerable proportion acted as subjects unwillingly. By the same reasoning, the acquaintances would probably feel least free to express directly to the experimenter whatever resentment they felt. Such resentment,

bottled within them, might cripple the desire or the ability to score well in their ESP runs.

The low ESP scores of the subjects tested in L.E.'s home may be dismissed as being due to chance factors or may be explained in terms of the inconvenience of travelling to L.E.'s home, the occasional interruptions, and the unrealistically close scheduling of appointments.

*Further Comparisons*

The many interrelations among categories still remain to be described. Was the difference between strangers and acquaintances tested at L.E.'s home more striking than the difference between those tested at the College? Were choices of their most successful ESP run better for egocentric or for task oriented subjects? Did the extrapunitive subjects make lower ESP scores on the runs where they reported having felt annoyed than did the impunitive? The number of such possible combinations is very large, and many of them have interesting implications. But they cannot, unfortunately, be described here. In almost all cases the successive fractionation required for comparisons of subdivisions would reduce the number of cases to a point where chance variations could not be disregarded. This experiment, therefore, can only raise the questions; it has not collected enough data to answer them.

## Summary and Conclusions

One hundred subjects were each required to make five standard ESP runs and were asked to make one additional guess, in which they stated on which run they thought they had made their best score ("second order ESP"). Two conclusions are outstanding:

1. Subjects who, as a group, failed to produce significant ESP scores by one method scored significantly when tested by another method.

2. Group ESP data that are not significantly different from chance can, if analyzed in terms of psychologically relevant variables, show that extrachance factors have determined the ESP guesses.

Psychological factors which appear to be related to ESP success in this experiment were: being task oriented rather than ego involved, believing in ESP rather than feeling doubtful about it or rejecting it completely, being predominantly impunitive or intropunitive rather

than extrapunitive, and the personal relation between the subject and the experimenter.

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# BOOK REVIEW

MATTER, MIND, AND MEANING, by Whately Carington. New Haven: Yale University Press, 1949. Pp. xx + 257.

The reading of this posthumous and, unfortunately, uncompleted work should prove both interesting and instructive to anyone —be his philosophical position what it may—whose mind is not closed to the reception of new ideas. The work is written, alas, in a flamboyantly bombastic and cock-sure style which will be likely to repel a fair-minded inquirer who is looking for a serious and courteous discussion of important and difficult problems. Mr. Carington pours unmitigated scorn upon all views other than his own, and the contemptuous references to professional philosophers which litter his pages reveal only too clearly the uneasy feeling of inadequacy which inevitably haunts the amateur. But the reader will be well advised to persevere despite the unfavorable impression produced thereby; for the author has undertaken a task which, if it can be carried out successfully, will prove of the greatest importance.

At the present day when Logical Positivism—despite its waning influence—still attracts an extensive following, and when in the domain of theology an extreme supernaturalism enjoys a wide popularity, it is only to be expected that some attempts at a rapprochement between these two types of irrationalism should be initiated—and, as a matter of fact, some have been. This, however, is not our author's concern. For the Theistic hypothesis he entertains no respect whatever. On the contrary, Mr. Carington is convinced that Logical Positivism is the only tenable theory, and that, except a man believe this, he shall—so far at least as his name and fame as a philosopher are concerned—without doubt perish everlastingly. Whatever may be thought of Mr. Carington's logic, of his positiveness there can be no question. None the less he is animated, inconsistently as it may appear, by the conviction that, unless a world-view which will afford a satisfactory basis for ethical and religious convictions can quickly be worked out, disaster is bound to fall upon civilization. How he would have treated the subject in detail

unfortunately we do not know. What is evident is the fact that he believed that Logical Positivism provides a theory in accordance with which the phenomena documented by parapsychology can be satisfactorily accounted for, and some sort of survival established. In other words, Mr. Carington aspired to do for us in the twentieth century what the early Buddhist philosophers attempted to do for their contemporaries over two thousand years ago, that is, to show how the affirmation of immortality can be reconciled with a philosophy of pure becoming.

This is a speculative adventure which may well intrigue even Mr. Carington's despised metaphysician. It is to be remarked, however, that the early Buddhists were in a better position to accomplish such a feat than is our author, for, although they anticipated him in rejecting the conception of substance, they did not reject the notion of causality as an intrinsic relation, and consequently, by substituting the latter for the former, were able to provide the self with a metaphysical structure. Such a maneuver is impossible for the Logical Positivist who is fully committed to the "regularity" view of causality, as Professor Ewing has termed it. This is explicitly recognized by Mr. Carington on page 106. "Statements involving the word 'cause,' etc.," he declares, "are in fact no more than convenient ways of talking about certain observable regularities in phenomena"; and the same is true of the kindred notion of "force," which is, he informs us, "a derivative conception of a fictional and strictly tautological character."[1] "We must in no circumstances," he writes, "allow ourselves to think of a 'force' as something capable of existing independently of the entities on which it is said to operate. All we know are cognitum groups and sequences, and any 'law of associational force' must be in the nature of a compendious generalization of the way in which these change and succeed each other."[2]

Nevertheless, despite the frequency with which Mr. Carington reminds himself of this pronouncement, he appears to forget it with equal frequency; and, if he does not habitually think of force as something "independent" of particular entities, he does certainly appear on numerous occasions to regard it as something in addition to, and regulative of, particulars. Thus "association" and "dissociation" seem to be something more than mere descriptive terms

[1] P. 165. [2] P. 170.

when he applies them to mental states; one feels that they refer to genuinely explicative principles. Furthermore Mr. Carington does not seem to realize that, if causality be no more than observed regularity of sequence, his own theories of telepathy and precognition are reduced to mere descriptive summaries, and are in no sense explanatory of these data.

According to the basic doctrine of Logical Positivism which Mr. Carington accepts, the only realities known to us are the data provided by sense, imagination, dream, phantasy, and hallucination—to which he applies the generic name of *cognita*—and the meaning of any statement *is* its verifiability in terms of these data. Since this theory is obviously incapable of verification in terms of its own criterion, it would appear that we have no recourse save to accept it as a dogma. Another dogma is the assertion that no propositions, other than tautologies, are self-evident. Is this proposition, one wonders, itself self-evident? However this may be, it is doubtless the case that one of the motives which has induced Mr. Carington to adopt the Logical Positivist position is an aversion to accepting "the bifurcation of nature"—that *bête noire* of Whitehead. Nevertheless the history of philosophy clearly shows that, although you can throw out the principle of bifurcation by the front door as often as you like, as often as you do so it will come in again by the back door. And so it is in this case. The dualism of mind and body has been ejected, together with the notion of substance—a notion unverifiable in terms of cognita. There are neither selves nor things; there are only cognita. Hence the problem of interaction has vanished. Cognita are all fellow-citizens of the universe and may associate with one another as they please.[3] Yet seemingly they do not please. "Words such as 'matter,' 'material object,' or 'material event,' " writes Mr. Carington, "refer to cognitum groups and sequences which follow each other in a certain type of pattern or with a certain kind of regularity, the type codified as what we call the laws of (material) physics. Words such as 'mind,' 'mental object,' or 'mental event' are cognitum sequences which follow a different sort of pattern, to wit those codified in the laws of psychology, so far as those are at present understood. There is no

[3] But no cognitum can exert efficacy upon another cognitum. The regularity view of causality outlaws such an assumption.

intrinsic difference between the cognita constitutive of the one type of sequential pattern and those constitutive of the other. The differences between the referents of the two sets of terms are solely differences of organization, not of the nature of the constituents."[4]

Quite so. There is no intrinsic difference of nature, but there is an intrinsic difference of pattern. Bifurcation has changed its *locus,* yet it is no less present. Mr. Carington, indeed, cherishes the hope[5] that ultimately the development of physics will make it possible to subsume both types of behavior under a single law; yet it is plain that such a law will leave intact the intrinsic differences of pattern, inasmuch as no theory can eliminate a fact. It is also clear that in accordance with Logical Positivism no theory could be advanced to account for these differences of pattern which would not carry us beyond cognita into the field of metaphysics, and so would, by definition, be nonsense. Obviously, then, to refuse to accept this bifurcation as ultimate would be to deny the faith.

Astonishingly enough Mr. Carington does, however, on two occasions qualify his allegiance to Positivistic orthodoxy. The first of the lapses occurs on page 136 when he concedes that "the entities known as cognita when they are cognized exist also as cognizables when they are not." This is metaphysics, sheer and shameless, inasmuch as it is evident that such an assertion can never be verified; and it is the more amazing since the orthodox answer that the very question is meaningless would have enabled him to avoid all discussion of the topic. The second lapse occurs on page 147 where he remarks, "I think that I shall be safe in taking it for granted as agreed that other people do have minds more or less closely resembling that to which I refer when I speak of my own mind; that they are conscious in much the same kind of way that I am; that they dream dreams, experience images, are occasionally liable to hallucination—and so forth"—and this in the face of Professor Ayre's declaration that the problem of other minds is to be treated behavioristically. However he atones for this lapse on pages 240-241 where he undertakes to define another mind in terms of bodily reactions, such as answers to questions. Would the respondent, one may ask, be likely to admit such a definition of his mentality as adequate?

[4] P. 229.

[5] P. 150.

The last query suggests a graver one. What is it that commends to Mr. Carington the method which he pursues? Its empirical character? But is it genuinely empirical? Is Hume, when he introspects and fails to discern the ego, more empirical than Descartes when he introspects and discovers it? Both men cannot be right. But is it immediately obvious which is at fault? Is introspection so easy a task that one can decide offhand? At least Mr. Carington should have examined and criticized McTaggart's profound discussion of the subject.[6] McTaggart's argument is too detailed adequately to summarize here, but briefly it is as follows: I am aware that I am aware of equality; therefore I am aware both of equality and of myself. This involves that the self which makes the judgment is identical with the self that is aware, and this identity could never be arrived at by inference but must be directly intuited. Here is an argument which is as empirical, and which to some of us seems as cogent, as any argument could be. It at least merits a refutation. If it be sound, it renders inevitable the conclusion that the self is more than a mere series of successive states.

I have raised the above objections to Mr. Carington's theory because I believe that they are both valid and important, and because I do not welcome the editor's, Professor Price's, suggestion that despite its defects it provides a convenient "working philosophy" for the psychical researcher, since it enables him to discard the notion of mental substance which "does not fit the empirical facts."[7] What at first sight appears more convenient may prove disastrous in the long run. Whatever may be the case with science, in philosophy the ultimate arbiter is, not convenience, but truth; and I should be sorry to see parapsychology think itself into a philosophy which it must subsequently think itself out of.

In conclusion, however, I wish to express my admiration for the ingenuity with which Mr. Carington, after taking as his point of departure a doctrine so apparently antithetical to the conclusion which he intends to establish, proceeds to work out a theoretical basis for a belief in survival. It is the discrepancy of the patterns of "mental" and "material" behavior, the principal of bifurcation

[6] McTaggart's argument will be found in his article on "Personality" in *Hasting's Encyclopedia of Religion and Ethics*, and also in the chapter on "Spirit" in the second volume of his *magnum opus, The Nature of Existence.*

[7] P. xii.

to which I have referred, which enables him to do this. In man's earthly life the field of consciousness always includes cognita some of which belong to one kind of pattern and some to the other, some that are somatic and some that are mental. But there is no inherent necessity in this; and it is quite conceivable that after death the field of consciousness will include only cognita which are united in a mental pattern. In other words the notion of mental pattern fulfills for Mr. Carington the function which the notion of causality as an intrinsic relation performs for the Buddhist.

I cannot say that I regard this theory as plausible—although it appears to me quite as much so as the premises upon which it is based—but its subtlety and originality claim my applause. Mr. Carington has pursued his line of thought with a resolution which is certainly heroic—although to some it may appear also regrettably misguided. While all philosophers may profit from reading this book, there are two groups to whom I would especially commend it—the Logical Positivists and those who, like the reviewer, are convinced of the basic soundness of a substance philosophy. It will give them, as it has given him, much to think about.

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# GLOSSARY

In order to avoid constant redefining of commonly recurring terms in papers appearing in this JOURNAL, the following definitions are submitted for convenient reference.

A simple description of the basic experimental methods and of the statistical procedures to be followed in evaluating ESP (both card tests and drawings tests) and PK may be found in the *Handbook of Tests in Parapsychology* by Betty M. Humphrey (published by the Parapsychology Laboratory, Duke University, Durham, N. C.).

AGENT: In tests for telepathy, the person whose mental states are to be apprehended by the percipient. In GESP tests, the person who looks at the stimulus object.

AVERAGE SCORE: Average number of hits per run.

BT ("Broken Technique"): The clairvoyance technique in which each card is laid aside by the experimenter as it is called by the subject. The check-up is made at the end of the run.

CALL: The ESP symbol selected by the subject in trying to identify a target.

CHANCE: The complex of undefined causal factors irrelevant to the purpose at hand.

CHANCE EXPECTATION = MEAN CHANCE EXPECTATION: The most likely score if only chance obtains.

CHANCE AVERAGE: Mean chance expectation expressed as an expected score, generally in terms of average per run.

CHI SQUARE: A sum of quantities each of which is a deviation squared divided by an expected value. Also a sum of the squares of CR's.

CLAIRVOYANCE: Extrasensory perception of objective events as distinguished from telepathic perception of the mental state of another person.

CR (CRITICAL RATIO): A measure to determine whether or not the observed deviation is significantly greater than the expected random fluctuation about the average. The CR is obtained by dividing the observed deviation by the standard deviation. (The probability of a given CR may be obtained by consulting tables of the probability integral, such as Pearson's.)

CR OF THE DIFFERENCE: The observed difference between the average scores of two samples of data divided by the standard deviation of the difference.

DECK: Twenty-five ESP cards.

CLOSED DECK: An ESP deck composed of five each of the five symbols.

OPEN DECK: An ESP deck made up of the ESP symbols selected in random order.

DEVIATION: The amount an observed number of hits or an average score varies from the mean chance expectation or chance average. A deviation may be total (for a series of runs) or average (per run).

DIE THROW: The elementary unit of data in a dice-throwing test represented by the throwing and reading of a single die, no matter how many dice are thrown at a time.

DISPLACEMENT: ESP responses to targets other than those for which the calls were intended.

BACKWARD DISPLACEMENT: ESP responses to targets preceding the assigned targets (the ones for which they were intended). Displacement to the targets

one, two, three, etc. places preceding the assigned target are designated as (−1), (−2), (−3), etc.

FORWARD DISPLACEMENT: ESP responses to targets coming later than the assigned targets. Displacement to the targets one, two, three, etc. places after the assigned target are designated as (+1), (+2), (+3), etc.

DT (Down Through): The clairvoyance technique in which the cards are called down through the deck before any are removed or checked.

EMPIRICAL CONTROL: An experiment which wholly or partially follows the main experiment with the exception that the conditions are designed to exclude the possibility of the hypothesis being tested.

ESP (EXTRASENSORY PERCEPTION): Response to an external event not presented to any known sense.

ESP CARDS: Cards, each bearing one of the following five symbols: star, circle, three parallel wavy lines (called "waves"), square, plus.

ESP SYMBOLS: See plate opposite page 1, this JOURNAL, Vol. 1, March, 1937.

EXPECTATION; see CHANCE.

EXTRACHANCE: Not due to chance alone.

FREE MATERIAL: The stimulus objects of experiments in which an unlimited or unspecified range of stimulus objects is employed (as contrasted with methods such as card-calling in which the subject knows that the stimulus object is one of a known range).

FREE RESPONSE METHOD: An ESP test method in which a relatively unlimited range of stimulus objects is used and in which the subject is instructed to express freely as his response any range of thoughts and ideas.

GESP (GENERAL EXTRASENSORY PERCEPTION): A technique designed to test the occurrence of extrasensory perception, permitting either telepathy or clairvoyance or both to operate.

HIGH-DICE TEST: A PK technique in which the aim of the subject is to try to influence a pair of dice to fall with the two upper faces totaling eight or more.

LOW-DICE TEST: A PK technique in which the aim of the subject is to try to influence a pair of dice to fall with the two upper faces totaling six or less.

MEAN CHANCE EXPECTATION; see CHANCE.

P (PROBABILITY): A mathematical estimate of the expected relative frequency of a given event if chance alone were operative.

PARAPSYCHOLOGY: A division of psychology dealing with those psychical effects which appear not to fall within the scope of what is at present recognized law.

PERCIPIENT: The person who makes the calls in an ESP test.

PK (PSYCHOKINESIS): The direct influence exerted on a physical system by a subject without any known intermediate physical energy or instrumentation.

PRECOGNITION: Cognition of a future event which could not be known through rational inference.

PREFERENTIAL MATCHING: A method of scoring free responses. A judge ranks the stimulus objects (usually in sets of four) with respect to their similarity to, or association with, each response; and/or he ranks the responses with respect to their similarity to, or association with, each stimulus object.

PSI: A general term to identify personal factors or processes in nature which transcend accepted laws. It approximates the popular use of the word "psychic" and the technical one, "parapsychical."

PSI PHENOMENA: Occurrences which result from the operation of psi. They include the phenomena of both ESP (including precognition) and PK.

QD (QUARTER DISTRIBUTION): The distribution of hits in the record page (or in a logical subdivision thereof, such as the set or the half-set) as found in the four equal quarters formed by dividing the selected unit horizontally and vertically.

RUN: A group of trials, usually the successive calling of a deck of 25 ESP cards or symbols. In PK tests, 24 single die throws regardless of the number of dice thrown at the same time.

SCORE: The number of hits made in one run.

TOTAL SCORE: Total of scores made in a given number of runs.

AVERAGE SCORE: Total score divided by number of runs.

SD (STANDARD DEVIATION): Usually the theoretical root mean square of the deviations. It is obtained from the formula $\sqrt{npq}$, in which $n$ is the number of single trials, $p$ the probability of success per trial, and $q$ the probability of failure. (For ESP cards, SD $= 2\sqrt{\text{no. of runs.}}$) Sometimes the SD used is that estimated from the observed variability in the scores.

SD OF THE DIFFERENCE: For both ESP cards and PK tests using dice, the SD of the difference is equal to $\sigma_s \sqrt{1/R_1 + 1/R_2}$ where $\sigma_s$ is the SD of a single run and $R_1$ and $R_2$ are the number of the runs in the respective samples compared. This gives the SD of the difference for run score averages.

SERIES: Several runs or experimental sessions that are grouped in accordance with a stated principle.

SESSION: A unit of an ESP or PK experiment comprising all the trials of one test occasion.

SET: A subdivision of the record page serving as a scoring unit for a consecutive group of trials, usually for the same target.

SEVENS TEST: A PK technique in which the aim of the subject is to try to influence a pair of dice to fall with the two upper faces totaling seven.

SIGNIFICANCE: A numerical result is significant when it equals or surpasses some criterion of degree of chance improbability. The criterion commonly used in this JOURNAL is a probability value of .02 or less, or a deviation in either direction such that the CR is 2.33 or greater.

SINGLES TEST: A PK technique in which the aim of the subject is to try to influence dice to fall with a specified face up.

STIMULUS OBJECT: The ESP card or drawing or other object, some identifying characteristic of which is to be apprehended by the percipient.

TARGET: In ESP tests, the stimulus object; or in telepathy, the mental state of the agent. In PK tests, the faces of the dice (or combination of faces) which the subject attempts to bring up in the act of throwing.

TARGET CARD: The card which the percipient is attempting to perceive (i.e., to identify or otherwise indicate a knowledge of).

SUBJECT: The person who is experimented upon. In ESP tests, most commonly the percipient (though also the agent in GESP and telepathy). In PK tests, any individual whose task it is to influence the objects thrown.

TARGET DECK: The deck of cards the order of which the subject is attempting to identify.

TARGET FACE: The face on the die which the subject tries to turn up as a consequence of direct mental action.

TELEPATHY: Extrasensory perception of the mental activities of another person. It does not include the clairvoyant perception of objective events.

TRIAL: In ESP tests, a single attempt to identify a stimulus object. In PK tests, a single throw of the dice or other objects thrown.

# THE PARAPSYCHOLOGY LABORATORY OF DUKE UNIVERSITY

The Parapsychology Laboratory of Duke University dates from 1930 when its research program was launched by members of the Department of Psychology under the sponsorship of Professor William McDougall and with the favorable interest of the President of the University—at that time, Dr. William Preston Few. The first report of the work done was issued in book form in 1934 under the title *Extrasensory Perception;* in 1937, the *Journal of Parapsychology* was established; and in 1940, five members of the Laboratory staff collaborated to publish *Extrasensory Perception After Sixty Years,* which summarized not only the work of the Laboratory but similar research, past and present, in other places as well.

*Problems of Research.* The main topic of investigation has been extrasensory perception in its various phases—telepathy, clairvoyance, precognition—and psychokinesis, or the direct influence of the mind over matter. Other work has been planned, and to some extent undertaken; but the main objective has been to arrive first at a fair understanding of extrasensory perception and psychokinesis themselves before turning definitely to other problems of parapsychology. However, an experimental staff is being trained and methods developed for a broad and inclusive program which awaits only the time and the necessary financial assistance for its execution.

*Financial Support.* The Laboratory is supported jointly by Duke University and a small group of donors. Thus far it has had no permanent endowment and has been maintained by current contributions. Its finances are handled by the University treasurer in a special fund known as the William McDougall Research Fund, which was established in 1935.

A GENERAL INFORMATION BULLETIN may be obtained by addressing a request to Box 6847, College Station, Durham, N. C.

A SECOND cause to hesitate today in accepting too rigidly materialistic a philosophy is the remarkable change which has come over the physical sciences since the century's turn. The good old days of billiard-ball atoms, Euclidean geometry, and the indestructability of matter are now gone. Materialism persists, but it is based upon a matter far more tenuous than the simple stuff on which the previous century built its confident theories. Matter in the old sense, indeed, has ceased to be and is replaced by a fantastic system of electrical charges, quanta and space strains, which are literally inconceivable and are open to analysis only by mathematical subtleties. The universe in which our fathers felt so comfortably at home has vanished. Although biologists and psychologists, beginning to experience the delight of dealing with life directly in terms of physics and chemistry, still are inclined to think in simpler and more dogmatic terms, the physical scientists, as one who reads De Nöuy, Eddington, Schrödinger, and many others will agree, are far less certain of the validity of the naïve materialism of a generation past. As Herbert Muller puts it, "The physicists have arrived at more satisfactory interpretations of experimental facts by scrapping self-evident truths, breaking the laws of thought—by a systematic exploitation, as it were, of the nonsense which the eighteenth century had triumphantly eliminated." No longer can one say, as Renan did not so many years ago, "Today there are no more mysteries." Whether we like to admit it or not, things are more mysterious than they used to be, a century of science notwithstanding. The plot of the great epic of the universe grows deeper with each new page we turn, and how it will conclude, what great catharsis will gather all its threads once more together, the wisest cannot say. Great things are in the air, exciting new ideas in the sciences which may still further modify our understanding of the universe. This is no day to be dogmatic or complacent. The idealist who follows the ancient highway of the spirit toward reality has gained a more respectful audience than was his a half a century ago, serious men who look to him now for something more than pleasant fairy tales.

—E. W. SINNOTT, "Science and the Whole Man,"
*American Scientist*, January, 1948.